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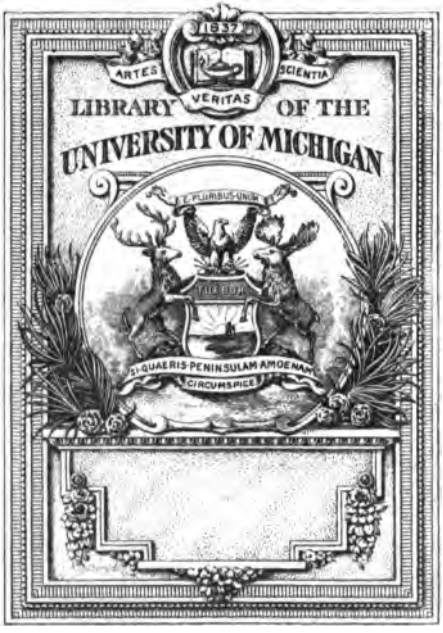
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FOR THE INVESTIGATION OF

EPIDEMIC DISEASES.

INSTITUTED AUGUST 1850.

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OBJECTS

OF THE

EPIDEMIOLOGICAL SOCIETY.

THE EPIDEMIOLOGICAL SOCIETY was instituted, in 1850, for the study of Epidemic and Endemic Diseases, with special reference to the investigation of (*a*) the various external or physical agencies and the different conditions of life which favour their development or influence their character; and (*b*) the sanitary and hygienic measures best fitted to check, mitigate, or prevent them.

Under the term "Epidemic" are included almost all the diseases which have of late years been classified under the head sometimes of "Zymotic," and at other times of "Miasmatic," diseases. The class comprises not only the complex group of Continued and Periodic Fevers, as well as the genuine Exanthemata, but also Diarrhœa, Dysentery, and Cholera, Hooping cough and Influenza, Diphtheria and Cynanche, etc., etc.

Many diseases usually occurring in isolated or sporadic cases, and arranged as "local diseases" or "diseases of particular organs," and "constitutional diseases" or "diseases of uncertain seat," assume at times an epidemic character; such as ophthalmia and certain diseases of the skin in workhouses, schools, prisons, etc.; infantile convulsions and tetanus in lying-in hospitals, etc.; erysipelas and pyæmia in infirmaries; scurvy, ulcers, and boils in ships.

Special endemic or indigenous diseases, peculiar and limited to certain districts or regions of countries, such as the goître and

cretinism of Alpine valleys; the pellagra of Lombardy and other districts of Italy, etc.; the leprosy of the West Indies, Spain, Norway, etc.; the beriberi of some parts of India—come strictly within the province of the Society's inquiries. More chronic in character, and more persistent from year to year than genuine Epidemic maladies, these Endemic diseases, being generally associated with a depraved or cachectic condition of the resident population, present an important field of hygienic research.

While the chief object of the other medical societies of the metropolis is the investigation of the physiological, pathological, and therapeutic relations of diseases, that of the Epidemiological Society is specially the study of their etiological or causal relations and the influences of locality, climate and season, diet and occupation, etc., etc., on their rise, dissemination, and continuance.

Diseases are looked at not so much in detail as in the aggregate; not in individual cases, but in groups and successions of cases; and not in one place only, but over wide and varied areas of observation. It is as a branch of Natural History rather than of technical medicine that the Society seeks to render the knowledge of epidemics and endemics more accurate and complete.

The range of inquiry comprehends some of the most curious and important problems which can engage the attention not only of the physician and philosophic naturalist, but also of the legislator and statesman, and indeed of all who interest themselves in what concerns the health and welfare of communities and peoples.

The question as to the *Genesis of the great group of Idiopathic Fevers* still awaits a much stricter and more elaborate investigation than it has yet received from the profession.

The part which *Contagion*, or the property of communicability from person to person, plays in the dissemination of various diseases, continues to be a fruitful source of discussion among medical men, and of perplexing anxiety to the public. In connexion with this question, the important international subjects of *Quarantine and Prophylactic Medicine* have to be considered.

The influence of the *Sanitary condition of places and dwellings* on the development and course of Zymotic diseases is another

important question. Happily, general attention is much drawn to this subject in the present day, and it is therefore very needful to enlighten and guide the public mind.

Some diseases are much more influenced by, and amenable to, ordinary sanitary precautions than others. To distinguish the one class from the other, and to determine to what extent the drainage of lands and towns, the removal of surface nuisances, the due supply of pure water, etc., affect the spread and malignancy of each, is a point requiring much consideration. Sanitary and preventive medicine has always engaged much of the Society's attention, and this too in respect not only of towns and villages at home and in our colonies, but also of the barracks and cantonments of our troops, of our ships both of war and commerce, and of our docks and harbours.

Topographical Medicine, although one of the most useful and attractive departments of medical research, has not yet been investigated with the care it deserves. We are still without any medical topography of our own country; and yet such a work could not fail to bring a rich harvest of practical benefits. The knowledge of the topographical distribution of epidemic diseases over the length and breadth of our land might lead to many useful as well as interesting results. Such an inquiry may well engage the attention of other medical Societies, in co-operation with the Epidemiological.

Similar inquiries are needed in the colonial and other distant possessions of the British crown. To take but one example:—What good might not be expected from a systematic topographical survey of India, through the agency of the medical officers, military and civil, scattered over every part of that empire!

The *Geography of Epidemic and Endemic Diseases* presents a still wider field for research, and is one which the Society is desirous to cultivate. It is of course but an expansion of topographical medicine, embracing not any one country or region, but the large divisions, and even the entire extent of the world's surface. Some of these diseases appear to be really 'pandemic,' in other words, to have invaded all, or almost all, peoples; while other members of the class have hitherto been limited to much

narrower bounds. It is the province of geographical medicine to collect and record all facts illustrative of this department of nosological research.

The *Chronology of Epidemics* is another subject which demands inquiry. Its importance was better recognised by some of the ablest physicians in the 17th and 18th centuries, than it has been during the present one. The want of reliable registers of the rise, progress, and continuance of epidemic visitations, in conjunction with accurate data as to the meteorological and other physical phenomena, prior to their advent and during their prevalence, is much felt by all inquirers. Such registers are indispensable for the progress of Epidemiology as a branch of science.

The systematic *Registration of Diseases*, as well as of the mere mortality resulting therefrom, among large sections of the civil population, and on the plan followed out in the annual Medical Returns of the Army and Navy, has been strongly urged on the Poor-Law Board by the Society.

A knowledge of *Epizootics*, or epidemic diseases among the lower animals, whether contemporaneous or not with such diseases in the human family, is also sought to be obtained. The subject of *Epiphytics*, or vegetable blights, etc., likewise demands attention.

It will thus be seen that the labours of the Society are directed to the investigation of many of the most important problems of State Medicine and of public Hygiene in their widest and most comprehensive acceptation, and throughout all countries and nations. The Epidemiological is thus truly an International Society.

For the successful prosecution of such works, the cordial co-operation of medical men, not only at home, but in all our colonies and possessions abroad, as well as in foreign countries, is most necessary.

The medical officers of the Army and Navy enjoy singularly favourable opportunities for the study of epidemic diseases, and of the circumstances attending their development, in the most different climates and regions. To these gentlemen, and also to their professional brethren resident in the scattered colonial and other possessions of the empire, the Council appeal with earnestness

for assistance in the task they are endeavouring to perform. No other country possesses within its own dominions so wide and so varied a field of observation as Great Britain. It behoves, therefore the medical profession to profit by such an advantage, for the cultivation of science, and for the promotion of the welfare and happiness of mankind.

The Ordinary Meetings of the Society are held at the Rooms of

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Subscriptions and Donations may be paid to the Treasurer, or Secretaries. The Annual Subscription of Resident Members is One Guinea. Non-Resident Members pay One Guinea on election; but they are not called upon for any further Contribution.

The Rules and Regulations of the Society, with an Exposition of its Objects, may be obtained from the Secretaries.

The Transactions of each Session are published and distributed to the Resident Members free of cost.

The Transactions for the Session 1859-60 *are out of print. Those for the Sessions from* 1860-61 *to* 1867-68, *inclusive, may be obtained from the publisher,* MR. ROBERT HARDWICKE, 192, PICCADILLY, W. *Prior to* 1859-60 *the Transactions were published in the "Sanitary Review."*

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EPIDEMIOLOGICAL SOCIETY.

ADDRESS DELIVERED AT THE OPENING OF THE SESSION 1866-67.

By Sir WILLIAM JENNER, Bart., M.D. Lond., D.C.L. Oxon., F.R.S.,

PRESIDENT OF THE SOCIETY; PHYSICIAN IN ORDINARY TO H.M. THE QUEEN; PROFESSOR OF MEDICINE, UNIVERSITY COLLEGE; PHYSICIAN TO UNIVERSITY COLLEGE HOSPITAL; FORMERLY PHYSICIAN TO THE HOSPITAL FOR SICK CHILDREN, ETC.

Gentlemen,—Since the commencement of the last Session, the physician who from its foundation in 1850 till two years since presided over the Epidemiological Society has passed from among us. Learned, and possessed of considerable intellectual power, accomplished and refined, unobtrusive and gentle in manner, the type of an amiable man, Dr. Babington was loved and respected by all acquainted with him. His powers, knowledge, and influence he was at all times willing to exert in the cause of humanity and his art. And no small additional praise of him it is to say that, although a practical and practising physician, he never wrote anything which can be supposed to have been dictated by other motives than a desire to advance or to diffuse knowledge, and that he wrote nothing which did not answer the end for which it was written. No one feels more strongly than I do how inadequately I can fill the place in this Society which Dr. Babington so long held, and which our second president so ably occupied. The time at my disposal is short—so short that I could not hope, had I the ability, to advance our knowledge. Nor should I have responded to the offer of the Council had I not been told that positions which I happen to fill would, in their opinion, render me of service to the Society when, as often happens, it has to communicate with other authorities. This evening I shall, it seems to me, be best performing the duty of my post by reviewing from a particular point of view—namely, from that which every

practitioner of medicine necessarily occupies,—very briefly and in general terms, that great group of diseases to the investigation of the laws of a single class of which this Society especially devotes its labours. I review the whole group, because the diseases constituting it, dissimilar as they are in their pathology, are so intimately connected in their etiology that the removal of the causes of one of the group would certainly and greatly diminish the mortality from all.

To cure disease was the great aim of medicine in the last century. The idea of the vast majority of the public on medical subjects is the popular expression of the views held by the physicians of the generation just gone. The present popular idea of the science and art of medicine is that it aims chiefly at the discovery of special cures for all special diseases. During the past few months the authoritative utterances of the daily journals have afforded the clearest proof of the truth of this assertion. What is the use of veterinary art if it cannot tell the public a cure for the cattle-plague? What is the use of the science and art of medicine if it cannot point out to us a cure for cholera? Such has been the substance of many articles in the journals, which are the reflex of the popular ideas of the day.

The asking such questions, as well as the filling of column after column of the same journals with alleged specific cures and discussions on specific modes of treatment of cholera and of cattle-plague, afford practical evidence of the ignorance of the public on medical subjects.

In regard to the cattle-plague, the trying proposed specifics and seeking new specifics was indeed grasping at a shadow, in place of taking the substance offered by one of the most distinguished, best-informed, and able of our veterinary surgeons.

No one acquainted with the present state of the science and art of medicine will for a moment question that to prevent disease is its first and most important aim. That this is the great aim of medicine is a statement made in the opening lecture of all teachers of medicine, and in all their subsequent teaching they never lose sight of the fact. We spend time at the bedside in elaborating the history of cases and establishing diagnosis, and hours in the dead-house, that we may throw a little more light on the mode of attaining this the great aim of our profession. Failing to prevent, medicine strives to cure; unable to cure, it strives to lengthen life, and by lengthening life often saves life; and failing in all these, it can still be of service to large numbers by alleviating suffering.

Diseases have been grouped under the heads preventable and non-preventable; and although there are a considerable number which belong to the debatable ground, and many which can only be diminished in frequency and severity, it is of practical importance that the fact of the existence of a great group of preventable diseases should be often and often—it cannot be too often—urged by our profession, collectively and individually, on the general public.

In his well-known, remarkable, and able paper, Dr. Greenhow attempted to estimate the undoubtedly preventable mortality from disease. He compared the death-rate of the healthiest registration districts in England with that of the least healthy, and then ascertained the diseases from which the excess in mortality arose. These diseases should, he argued, be able to be so reduced in frequency in the unhealthy districts as to bring down the death-rate of the now unhealthy districts to a level with that of the healthy districts —to reduce the 36 death-rate of Liverpool to the 17 death-rate of group C.

There is, however, another mode of estimating generally the enormous amount of life sacrificed unnecessarily—*i. e.*, of deaths from preventable diseases—viz., to consider what diseases experience has proved to be due to definite, well-ascertained causes, and then, having regard generally to the direct and indirect mortality of those diseases, to examine how far their causes are practically removable. Much of this figures cannot express—the Registrar-General's returns cannot show. He who practises medicine on a large scale among the poor alone can appreciate it, and that necessarily only approximately and in a general way.

First among preventable diseases I will place one, the mortality from which, in London, at least, is so great, as beyond question to swell largely the death-rate of children under two years of age, and yet one that has no place in the Registrar-General's returns. I mean rickets, the English disease, as it was formerly called. Not one child *ought* to die from rickets itself, and death from its consequences ought to be extremely rare; and yet the mortality from rickets, from diseases which would not occur but for the pre-existing rickets, and from diseases which would be trifling but for the co-existence of rickets, is enormous. Laryngismus stridulus, chronic hydrocephalus, teething, convulsions, atrophy, disease of spleen and of liver, remittent fever, tabes mesenterica, spinal disease, bronchitis, diarrhœa, measles, whooping-cough,—these are some of the names under which deaths

really due to rickets appear in the Registrar-General's returns.

Syphilis, like rickets, is a preventable disease. The mortality from syphilis is far greater than appears in the Registrar-General's returns. It is only in recent times that physicians have begun to appreciate the gravity of the chronic constitutional consequences of this affection on the individual and on his offspring. The frequency with which cases of hepatic disease, of so-called tubercular phthisis, of Bright's disease, of brain disease, are referable directly to syphilis, and the many cases of so-called strumous disease in the child due to inherited syphilis, becomes daily more apparent.

Strumosis and tuberculosis are induced in a large proportion of cases, and the development of tubercle determined in a still larger proportion of the cases of unavoidable tuberculosis, by preventable causes. Strumosis and tuberculosis have no place in the Registrar-General's returns; and yet to the pre-existence of these diseased states, in a large proportion of cases, is due the death in scarlet fever, in measles, whooping-cough; and but for these states how large a number of cases of Bright's disease, hepatic disease, and puerperal mischief would never have occurred.

Delirium tremens and cirrhosis of the liver are as certainly preventable diseases as are syphilis and rickets.

Gout and its concomitant, Bright's disease, etc., are the consequences, in a large proportion of cases, of preventable conditions.

Of scurvy and the diseases due to the injurious influences consequent on the exercise of certain trades, I will not now speak. They are too numerous to be considered even briefly on such an occasion as this. That they are to a great extent preventable, no one can doubt.

Passing from these, we have, lastly, those preventable diseases in the consideration of which our Society is especially interested—viz., the whole class of zymotic diseases.

The causes exciting or permissive of all the preventable diseases I have enumerated are—impurity, excess, or deficiency of food, air, clothing, light, or exercise, sexual excitement, and inherited tendency. As the individual, society, or law can regulate or guide the supply of these agents of health and of disease, to some extent at least, it is clear that to that extent diseases due to them are preventable. Although no one of these diseases could, by the spread of practical knowledge, by care or watchfulness on the part of society, or by any law, or, with the exception of one, by any moral change

in individuals, be altogether prevented; although, in the best conceivable condition of society, cases of these diseases would be present among us, the mortality from them might be enormously diminished, and thus the annual mortality be reduced far below the ideal of perfection of the Registrar-General—17 per 1,000 living.

There are three modes of effecting this diminution:—

1st. By the spread of the knowledge of sanitary laws. Practical knowledge of this kind can be spread only by constant reiteration. One constantly meets persons, accomplished and highly educated, who would be ashamed to be ignorant of classical and mathematical knowledge, and are yet even now practically ignorant of the simplest sanitary laws. Not long since, I saw a body of men lodged for months, by the authority of highly educated gentlemen, in rooms where each man had considerably less than two hundred cubic feet of breathing space. Again, who is not acquainted with towns springing up under the fostering care of rich and influential proprietors, without any other mode of drainage than the collection of the filth of each house into its own cesspool, and no other supply of water than surface-pumps. Death on a large scale must be the necessary result, and that before the Sanitary Act of 1866 can prevent it. These things could not be if the public had practically realised the truth and force of sanitary laws. No one could then act thus. None could sin from ignorance, and the force of public opinion would be too strong for greed to give power to a man or even to a company to withstand it. Who, knowing the importance of the study of sanitary laws, does not heartily join in the "hope that one day our University will teach the doctrines of public health, and recognise those doctrines in granting degrees"? Line upon line, precept on precept, example on example, must be placed before the public. In season and out of season, we must force the subject forward. We must consent to be considered bores, as all sanitary reformers have been, if we would have the public so indoctrinated with the science of health as to carry its laws into practice.

2nd. By the force of special legal enactment. When a man's acts are calculated directly to endanger his neighbour's health, then the law should interfere to restrain him from the commission of those acts. Again, when the united simultaneous action of many is required for the performance of some work essential to the public health, the force of law is necessary, in the great majority of instances, to secure such action.

3rd. By the philanthropic efforts of the wealthy, the health of the poor may be directly improved, and indirectly the death-rate of rich and poor diminished.

I will now consider the several preventable diseases just enumerated, and endeavour to apportion to each its causes, and so to estimate to what extent inevitable conditions, want of acquaintance with the present state of sanitary knowledge, the indifference of society, and defect in our laws are severally answerable for the deaths from these diseases. By doing this we shall gain a position from which to judge how far or to what extent these diseases are likely in years to come to be prevented in practice.

The causes of *rickets* are, poorness of the mother's blood, errors in diet, *i. e.*, feeding the child with food unsuited to its wants and to its digestive powers—and, as subsidiary causes, deficient light and impure air, produced especially by overcrowding of the sleeping room. Poverty—inevitable poverty—plays a great part in the production of some of these causes. If society did its duty in providing suitable abodes for the poor they would suffer little from the want of light or overcrowding at night. The anæmia of the mother would be less, and her blood better fitted to nourish the infant. Ignorance of the proper mode of feeding the child assists in the production of rickets in a larger degree than poverty. Judging from my own experience, I should say that rickets, so severe as to lead even indirectly to death, would be comparatively rare did the poor know how to feed a young child—were the poor aware of the necessity of the infant being fed with food fitted to its age. Law can do something here; for it can make compulsory the teaching of the *practical* laws of health in all schools supported in any degree by the public money. To teach young girls how not to destroy their future children, is surely as important as to teach them much of what is now considered essential for them to know. I would have an infant nursery attached to every national girls' school, so that the girls might be practically taught how to fulfil their practical duties to their family and to society. Diffusion of practical knowledge is the great preventive remedy of rickets. Law can aid in the spread of that knowledge, and society, if it did its duty, would remove the subsidiary causes of want of light and overcrowding. Inevitable poverty might possibly still keep rickets in a grave form among us, but were rickets kept within unpreventable limits the death-rate of infants in London would be perceptibly diminished.

Syphilis.—[illegible]

Acute Rheumatism.—[illegible]

Diseases.—[illegible]

Treatment.—[illegible]

losis is transmitted from parent to child, is one of the best established facts in medicine. The extreme frequency of tubercular diseases in some circumscribed country districts is, in part at least, explicable by the frequency of intermarriage amongst persons living in such districts; and, conversely, the exemption of particular circumscribed districts from tubercular disease is in part due to the same cause. In the one case, from some special circumstance, tuberculosis has been introduced into the district, and then spread in it from the cause I have mentioned—*i. e.*, intermarrying. In the other case, the freedom from the disease of the district at any given time is the cause of its continued freedom. Intermarriage of the inhabitants, the disease being present, spreads it far and wide; intermarriage of the inhabitants, the disease being absent, prevents its introduction. Dr. Christison, in his able address to the Sanitary Association in 1863, omitted, it seems to me, to give its due weight to this circumstance when estimating the causes of the relative frequency of tubercular disease in different localities. Very little can be done to prevent so much of tuberculosis as is due to transmission from parent to child. The spread of knowledge may do a little. But, seeing the frequency with which tuberculosis in its most intense form is produced in the children by the intermarriage of men and women as well acquainted with the facts of inherited disease as we are, the hope of diminishing tuberculosis by the spread of this kind of knowledge is indeed small. As to the causes of acquired tuberculosis, they are errors, especially deficiency of food, clothing, air, exercise and light, and previous diseases. Tuberculosis may be developed from these causes; but the preventable death-rate is especially increased by the effects of these causes in determining the formation of tubercle in those who inherit tuberculosis, and it may be only in trifling degree. Doubtless, poverty, inevitable poverty, necessitates the exposure of large numbers to these causes of tuberculosis and tuberculisation. But as in rickets, so in tuberculosis and tuberculisation. Ignorance of the proper mode of feeding her child, of the necessity for it of fresh air and of light, leads the mother to induce or intensify those diseases in her children—to cause the death of her child, or to render it liable as it grows older to be affected by causes which otherwise would have been innocuous. Society might do very much by providing the poor with residences, and with grounds in cities for exercise, where they could have light and air. It is not charity that is asked; but the rich can do

that for the poor which no forethought, no economy of their own could effect. Capital is required. Law can do much by enforcing proper practical sanitary education, by regulating more perfectly workshops—*i. e.*, by inspecting, &c. Machinery must be properly protected, so that no life may be lost by the careless or unwary being entangled among its wheels; but for every one that could, if no protection were enforced, be quickly killed or mutilated, tens of thousands are killed or reduced to ill-health, and pass their acquired disease to their children, by the want of stringent and efficient laws regulating the size, ventilation, &c., of our workshops. The Sanitary Act of 1866 may do a little to remedy this want; but, unless amended and made far more stringent and easy to put in force, it will do *very* little.

The other remaining diseases which may be in part or altogether prevented are the so-called *Zymotic*. The majority of these are contagious, and all have a specific cause.

Our knowledge has advanced so far as to enable us to say that, for the spread of contagious diseases *generally*, four things are essential:—

1st. The contagious or zymotic substance: be this germ or seed, animalcule or vegetable, or a merely chemical compound.

2nd. A condition of atmosphere or of surroundings permitting the life or stability of the chemical constitution of the contagious or zymotic substance.

3rd. A vehicle for the conveyance of the contagious or zymotic principle to the individual, or rather to the part of the individual from which it can enter his system.

4th. An individual to receive the contagious or zymotic substance whose body is in such a condition that in it the contagious substance—germ, seed, animalcule, or mere chemical compound—can multiply itself.

Our knowledge enables us to state, moreover, that any one of these conditions may be so modified by art as to favour or stay the spread of contagion.

As special diseases, the zymotic or contagious substance of which is certainly always about us, spread at times easily, rapidly, and to a vast number—*i. e.*, become epidemic,—and then, having reached a certain degree of prevalence, shrink to their former dimensions, one must admit that either changing conditions of atmosphere or of surroundings give increased activity to the contagious or zymotic substance or that usually they destroy or weaken the contagious matter and now fail to destroy its life or stability of constitution; or that external conditions preceding or present have so modified

the system of individuals as to render them more susceptible to the action of some one special zymotic substance.

With reference to the spread of zymotic diseases.

1. While we know nothing of the nature of the contagious or zymotic substance of any one of them, we do know that it is present in a special state of activity in different excreta in different diseases: thus, in the gastro-intestinal secretions in typhoid fever, cholera, and cattle-plague; in the skin and throat secretions in scarlet fever; in the throat secretion in diphtheria; in the pustular matter secreted by the skin in small-pox; in the discharge from wounds in pyæmia.

2. We know that special conditions of their surroundings have a great effect in favouring the spread of certain zymotic diseases. Thus a temperature of thirty-two degrees destroys the poison of yellow fever; free dilution with fresh air renders the contagious matter of typhus innocuous.

3. Vehicle. It is now placed beyond question by the labours of Dr. Snow, confirmed as they have been by experience, that water is one of the great agents in diffusing cholera; and, if possible, it has been more unequivocally proved that typhoid fever is carried from individual to individual in the same way. Pyæmia, ichoræmia, and septicæmia spread in wards by the morbid secretions of one patient being applied directly to some raw surface on another. The cattle plague contagious matter, being especially contained in the intestinal excreta, was conveyed by any substance to which that could adhere.

4. Individuals. Whatever lowers the tone of the nervous system renders men peculiarly susceptible to the action of the contagious substance of typhus—*e. g.*, famine, anxiety, fear. Persons suffering from scarlet fever and from Bright's disease are in a state favourable to the occurrence of pyæmia; those who have recently lived in a cold climate suffer more certainly than do those coming from hot climates when exposed to the zymotic principle of yellow fever. Dr. Beale states that he has recently observed in fatal cases of cholera certain changes of structure in the intestine which must have occurred before the attack itself.

As to the possibility, degree, and modes of prevention of the zymotic diseases.

The zymotic element of some diseases that from time to time burst out with violence—*i. e.*, become epidemic—is always present. The epidemic could not occur unless the zymotic element were there. So that one great measure of prevention is the destruction of the zymotic element as fast

as created by chemical agents. The general and perfect use of disinfectants can only be secured by the diffusion of knowledge—especially in regard of the excreta in which the zymotic element is most concentrated,—and by the intervention of the law. Law has attempted to secure their use in some cases by a clause in the Sanitary Act of 1866. But their best action can only be secured by the diffusion of practical knowledge. Air and water are two most easily applicable and powerful destroyers of the commonest zymotic substances. Society and law can secure a supply of these for every house.

In regard of the zymotic elements of diseases not always amongst us. By strict quarantine laws these may be excluded. The great preventive scheme of the Conference on Cholera that met at Constantinople was strict quarantine, especially between India and Europe—a most vexatious, most costly, and most impracticable scheme. At a far less cost the hygienic condition of Great Britain might be so much improved as to prevent the spread of cholera, even were its zymotic element constantly among us. And then the improvement in our hygienic condition to such a degree would not only prevent the spread of cholera, but diminish the mortality from almost every other preventable disease. Strict quarantine should, it seems to me, be enforced, in the present state of the world, only in regard to those diseases of which improved hygienic conditions cannot prevent the spread.. The cattle-plague seems to be such a disease.

As to the surroundings, it has been clearly shown that many conditions unequivocally removable favour the spread of contagious diseases. Society has its part to perform in providing proper dwellings for the poor—*i. e.*, dwellings proper in regard of air, light, drainage, and water-supply. Law can do much to improve surroundings by enforcing general drainage and constant and sufficient supply of pure water. Great influence can be exerted in checking the spread of the zymotic diseases by subjecting to strict supervision the special vehicles by which each is known to be conveyed from individual to individual. It is beyond dispute that water has been in many cases the great agent in the rapid propagation of cholera and typhoid fever over a district. It becomes more probable with every searching investigation into local outbreaks of cholera and of typhoid fever that these outbreaks, so terribly destructive to life, are the result of the admixture of the zymotic element of the one or the other with the drinking water, or of collections

of fetid gas, or of such close packing of the inmates of the house, and such close relations of the inhabitants to each other, that the excreta of the first person affected is taken into the stomach of others.

The application by law of a Sanitary Act capable of being worked from a central authority by the means of inspectors, is, it seems to me, the great remedy. Inspectors must precede, and not follow, the outbreak. Dr. Seaton's late investigations into the outbreak of typhoid fever at Tottenham show the importance of such a course. The inspectors, and not the inhabitants, should set the law in motion. As the Act is now practically worked, death may sweep away half a town or village before the Sanitary Act is put in force. As every case of a zymotic disease is a centre from which fresh cases may spring, the country at large is interested in the sanitary state of every town, every village, every house. The health condition of the town therefore should be regulated, not by the will of the inhabitants, but by the will of the whole community—*i. e.*, by some central authority.

The great objects, then, in the prevention of the present mortality from zymotic diseases are—

To exclude or destroy the zymotic element.

To improve the surroundings, so that the conditions may be less favourable for its development, and that fewer individuals who suffer may die.

To watch carefully the vehicles by which experience has shown the zymotic element may be conveyed to others.

To render individuals less susceptible to the influence of the zymotic element.

These objects could be attained in a great measure, and so the mortality from zymotic diseases be very greatly diminished,—

By the spread of practical sanitary knowledge.

By society providing proper residences for the poor.

By law really and at once enforcing proper drainage and water-supply and preventing overcrowding.

Numerous points in relation to the zymotic diseases have to be cleared up before we can reach that position in regard of their prevention which we may hope one day to attain.

It is only by slow degrees that the profession has succeeded in separating some of those diseases from each other—small-pox and measles, measles and scarlet fever, rubeola notha and rubeola; and now, for the prevention of the zymotic diseases, one of the most important problems in practical medicine has to be solved—namely, the identity or non-

identity of several of these specific diseases with others which resemble them in some of their most prominent symptoms.

Cholera is contagious; on this point there is no longer question. But what is the specific-cause relation between cholera and choleraic diarrhœa, and between severe summer diarrhœa and choleraic diarrhœa? Is cholera, in the form of choleraic diarrhœa, always amongst us? Again, what is the specific-cause relation between angina and diphtheria—between catarrh and influenza—between dysentery and typhus? We know that the zymotic element which produces contagious pyæmia may be generated in the frame of man *de novo*. A most important problem to be solved is that of the spontaneous origin of other zymotic diseases. The researches on the cattle plague have demonstrated how small a quantity of the zymotic element adhering to fomites may produce that disease in previously healthy animals. Again, the Cholera Conference considers it proved that the evacuations of cases of cholera so mild as to merit the name of choleraic diarrhœa only can communicate cholera; as modified small-pox may be the most virulent form of that disease.

Other points that require establishing are—

To what extent the conditions preceding an epidemic influence the occurrence of that epidemic.

The different effect of different quantities of the same zymotic element.

The differences in the degree of contagiousness of contagious diseases at different stages of those diseases.

The period of incubation of each, and the circumstances which shorten or lengthen that period.

The duration of the stage of invasion.

To collect evidence on these points and others of similar kind would be well worthy the labour of this Society.

The value placed by a community on individual life is one of the great tests of the state of its civilisation. It speaks but little for our advance in civilisation that the death-rate in England, due as it is in so great a degree to preventable causes, should still be what it was twenty years since—that the death-rate of the ten years '41-'50, and the death-rate of the ten years '51-'60, should be nearly the same. It speaks but little for our advance in civilisation that the State at once spent an enormous sum and made most excellent laws when oxen suffered, but spends with a niggard hand and frames the most inefficient laws when the lives of men are at stake. What will the Cattle-plague Commission &c.

have cost? What has been spent in the investigation and prevention of cholera?

Allowing for deaths due to degenerations, to unavoidably deficient and impure food, to unavoidable want of clothing, to unavoidably impure air and defective exercise, to individuals' own vice, prejudice, and greed, to zymotic causes which no forethought or care could avert, there still remains an enormous preventable mortality due to ignorance of the simplest laws of health, of the necessities of the body in regard of air, light, exercise, and food—to the neglect of their duties by the wealthy, who know and obey those laws, but fail to give the poor the means of obeying them (short-sighted that they are: the zymotic disease that carries off the rich man has commonly first flourished and gained strength among the poor)—to the inertia of the Legislature, its unwillingness to interfere with individual action, its fear of touching vested interests, its dread of offending religious prejudices, though they be the offspring of ignorance. If of money matters the following could be said, how much more true is it of sanitary. How strictly it applies to all the Sanitary Acts yet passed.

"English legislation abounds in abortive expedients. It shrinks from difficulties. There is very commonly an admitted evil, and very obviously only one real remedy. But very often that real remedy is painful, and if public attention is but half roused to the subject, we are apt to put up with some half-measure, which gives little or no trouble, which looks as if it might mend matters a little, and which has no disadvantage save that it is not a searching cure of the evils to be remedied, and that in a little while it will be forgotten on account of the slightness of its effect, while the malady itself will rage as much as ever."

Our duties are clear in this matter, both as individuals and as a Society—namely, to carry out in regard of other diseases what we have to some extent aided in effecting with reference to small-pox: to collect evidence, to disseminate knowledge, to spur on those in power to frame laws adequate for the occasion; to note every local outbreak of epidemic disease, and, either as individual members of the Society or by committees, to investigate or stimulate others to investigate the causes of such outbreaks, and then to spread the knowledge thus gained.

ON THE INTERNATIONAL SANITARY CONFERENCE, AND THE PRESERVATION OF EUROPE FROM CHOLERA.

By E. GOODEVE, M.B.,

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[*Read December 2nd*, 1866.]

Mr. President and Gentlemen,—At the request of your secretary, Mr. Radcliffe, I have drawn up for submission to the Society, a sketch of the principal recommendations of the International Sanitary Conference of Constantinople for preventing the diffusion of epidemic cholera, and for the preservation of Europe from future invasions of the disease.

The Conference has concluded that epidemic cholera originates entirely in India, and never in Europe; that it is a transmissible disease, following in the wake of man, and not carried by the atmosphere to long distances; that it is spread chiefly by the evacuations of choleraic patients; and that it rages most in localities which are in bad sanitary conditions. It believes that the cholera-poison may adhere to the surface of clothes, walls of houses, insides of ships, etc.; and that these may be the means of communicating the disease to persons coming within their reach.

Starting from these principles—the grounds of which it would be impossible to enumerate in the compass of this paper,—it has based its measures of preservation; and asserts that Asiatic cholera is to be met by measures of restriction of intercourse when practicable, by measures of purification and disinfection, and by measures of hygiene.

In the application of these measures the Conference has held in view the following objects—viz.: to prevent the development and the spread of cholera in its place of origin—in India itself, and its exportation therefrom; and, in order to preserve Europe, to check its advance westward from India; and to limit its diffusion in Europe should fresh invasions occur.

The Conference has not always gone into very minute

details; but has rather contented itself with indicating in a broad manner the nature of the measures to be taken, leaving much of the minutiæ to be settled by the local knowledge of those who will apply them.

Inverting the order of enumeration above mentioned, I will speak of the recommendations in the following order:—

1. Of the sanitary measures.
2. Of the measures of purification.
3. Of the measures of restriction.

Sanitary measures.—The Conference recognises in the fullest manner the absolute importance of attention to the sanitary condition of localities as a means of checking the development of cholera epidemics; and adopts the principle that against these safety for populations is to be found in the purity of their air, in the goodness of their drinking-waters, and in the cleanliness of the soil on which they live. In accordance with these views, it points out the necessity of sufficient space for dwellings, of ample room for lodging, and of free ventilation with air that shall be pure. To obtain the latter, it shows that, space and ventilation being secured, the purity of the atmosphere depends upon the cleanliness of the surface of the soil, and the non-impregnation of its substance with organic decomposing, and especially excrementitious matters. It believes that it is vain to expect that a town shall possess a pure atmosphere, one in which the choleraic poison has little tendency to multiply, if the soil is loaded with these matters. Hence the greatest care should be taken to prevent its infiltration with such. For this it asks for the abolition of all privies, with cesspools or wells, and the substitution for them of movable receptacles, such as earth-closets or "fosses mobiles," which can be frequently emptied and cleaned, and which may be so arranged that fæcal matter cannot soak into the soil. The contents should be carried out of the towns. The Conference objects to the system of drains or sewers in communication with houses, holding that in practice we cannot prevent the diffusion of noxious gases through the houses in connection with them, and that in times of cholera drains may spread the disease along a line of houses in communication with them; and that, owing to the porosity and rapid decay of masonry work, they readily allow of the impregnation and saturation of the ground through which they run with decomposing organic matters. When used, sewers should never be allowed to empty themselves into rivers, and waterclosets should never be within the houses themselves. Burial-grounds

should not be permitted within towns. Slaughter-houses and noxious trades should be forbidden. Not only should it be an object to prevent all human organic or excrementitious matter from penetrating into the soil of towns, but every sort of organic refuse should be speedily removed, before the atmosphere can become contaminated by their decay above or under the ground. In short, for a healthy town, no decomposing organic matter should penetrate its soil or rest upon its surface. As the decomposition of organic matters is retarded in a dry soil, towns should always be provided with surface, and where necessary with subsoil, drainage. As seaports are frequently the first places to receive arrivals from infected regions, it is obvious that it is of the highest importance to place them in healthy states, even more than it is for inland towns. The absolute necessity for pure water, and the great danger of the use of water contaminated by choleraic matters, are pointed out. The liability of contamination of the water of wells or streams near or among which cesspools or sewers lie is shown, as well as its unfitness for use under any circumstances, and especially during cholera epidemics. It proposes filtration or boiling for water containing ordinary impurities, where none other can be had.

The sanitary conditions insisted on by the Conference demand for their perfection the exercise of public and private hygiene. It may be doubtful how far in the present state of society private hygiene will assist in the great work; but all that may be called public hygiene in these recommendations may certainly be carried out, to the great advantage of the populations concerned. Public hygiene can regulate the width of streets, the space for habitations, the space for individuals, the direction and the termination of sewers and drains, the description of privies, the scavengering of towns, and the provision of pure water. All these are the prime agents in combating the conditions in which cholera epidemics flourish; and if they could be faithfully carried out, as they might certainly be, the shortcomings of private hygiene would be of comparatively little moment. The Conference has not ignored the difficulties of obtaining the desired state; but it looks forward to the time when, to quote its own words, "the minds of all men shall be penetrated with the truth that most endemic and epidemic diseases owe their violence and their spread to the massing together of people, and to the fatal customs prevalent among them. Then all will understand that it is in the power of man, at

the same time that it is his duty, to overcome by his efforts that condition which he has created by his ignorance." These measures are not to be taken only when cholera epidemics are near, but should be acted on at all times and in all seasons. They should be made permanent conditions. They may be costly, and they are in opposition to the system of sewerage in practice in England. It is probable, however, that reflection will show, and time will prove, the much greater security and efficiency of systems which seek to maintain the soil free from dangerous matters over those which allow them to permeate its substance. I will conclude this brief outline by saying that the Conference believes that if man is to be preserved from cholera epidemics, he must live on a clean soil, drink pure water, and breathe a sufficiency of pure air, and that under these conditions cholera will rarely rage around him.

The sanitary recommendations are not confined to towns, but extend to ships, for which the importance of the limitation of the number of passengers, free ventilation, and provision of pure water free from all taint of choleraic discharges, are fully insisted on in the recommendations on naval hygiene and its appendix.

Measures of purification.—To destroy the poison adherent to surfaces, and to prevent these from becoming sources of danger, disinfection and purification of houses, ships, clothing, etc., are required, and recommended upon the same principle as they are for other contagious diseases; but, in the opinion of the Conference, cholera requires in addition the chemical disinfection and destruction of all cholera evacuations, both of confirmed cholera and of diarrhœa. These should always be disinfected separately from common excreta, and never mingled with the contents of common privies or drains. They should be buried deeply out of harm's way, and where they cannot infect drinking-water. The Conference recommends that a general disinfection of drains, sewers, privies, and cesspools should be carried out by authority throughout the duration of a cholera epidemic, and until its extinction; that this disinfection should begin in a town immediately that the cases of diarrhœa in the track of an epidemic show the least tendency to increase; and that the use of all common privies should be forbidden to diarrhœal patients during the prevalence of cholera. All linen soiled by cholera evacuations, or which has been in use by cholera patients, should be plunged immediately into solutions containing chloride of zinc or lime, or chlorate of

soda, and remain immersed for twenty-four hours before washing, which ought to be thoroughly done, and combined with boiling of such articles; and that all articles which cannot be so treated, such as bedding and thick materials, should be burnt. The temporary abandonment of infected houses, barracks, and dwellings of all kinds, and their purification by free ventilation, sprinkling or washing the walls with solutions of chloride of lime or carbolic acid, and by the diffusion of sulphurous or nitrous acid gas, or chlorine gas, and whitewashing, should invariably be carried out; and several days, at least eight, should be allowed to elapse before their reoccupation. Measures of similar character are recommended for contaminated ships.

The purifying agents most relied on are chloride of zinc, sulphate of iron, carbolic acid, sulphurous-acid and nitrous-acid gases, free ventilation, and a temperature of 212 to 250 degs. Fahrenheit, for articles that can be submitted to it without damage. Certain kinds of goods, such as old stuffs, rags, skins, etc., should be also disinfected. It is not proposed to submit general merchandise to disinfection. The bales of goods, however, have to be landed when a ship has to be disinfected.

The opinion of the Conference on measures of disinfection and purification is expressed in the following resolution:—"That measures of disinfection applied to cholera on a well-arranged plan, and with perseverance, offer themselves as a powerful auxiliaries—1st, for diminishing the liability of a locality threatened with cholera; 2nd, for destroying the germ of the disease; and 3rd, for limiting in certain favourable circumstances, the spread of the epidemic.

Measures of restriction.—A belief in the propagation of cholera by human intercourse naturally leads to recommendations of measures for limiting such intercourse, and these have been proposed by the Conference. It considers restriction as regards *cordons sanitaires*, isolation of the sick, complete interruption of communication between infected and healthy places, and the modified interruption known as quarantine. Restriction of intercourse has to be considered as regards land and sea, and, as applied to ships and passengers, demands the machinery of bills of health, inspection previous to embarkation at infected ports, lazarettos, and means of purification of contaminated or suspected objects.

With regard to sanitary cordons, the Conference concludes that these, established in the midst of numerous and dense

populations, are uncertain in effect; but that, on the other hand, employed over limited districts, or in countries in which the population is thinly spread, as in certain countries in Asia, cordons are of great use against the extension of the disease.

With regard to isolation, it believes that when it can be applied to the first cases which mark the beginning of an epidemic it is a measure of prudence which no country desirous of its own safety should neglect.

With regard to total interruption of communication, it recognises this as efficacious if practicable, but admits the impossibility of carrying it out in general; and contents itself with stating that interruption of communication is the best method of isolating cholera centres, and that in consequence it should be applied in all cases in which circumstances permit of its efficient performance, but that this measure—only applicable over limited districts—becomes impracticable and inefficacious when an epidemic is spread over a large space.

On the question of checking emigration from infected towns to surrounding places, the Conference recognises the great evil of the flight of masses of people from the seats of epidemics, and its influence in spreading the disease. It It thinks that, without preventing persons moving from infected towns, it would be well if they could be restricted to limited areas around these diseased localities. With regard to people moving away by sea, it would limit the number of passengers in each ship, and submit them to medical inspection and their goods to disinfection before embarkation.

Quarantine is to be considered in its land and sea aspects. Land quarantines, from what has been already remarked, have not been often applicable; but in cases of caravans, troops, and masses of emigrants, it may be found occasionally very valuable. Maritime quarantine, however, on account of the greater facility of maintaining it, is considered to be much more likely to be efficient than that on land, and the conclusion was adopted that these quarantines, established on a rational basis and in conformity with the progress of science, may serve as effectual barriers to the invasions of cholera.

The quarantines recommended are of two kinds—quarantine of observation and strict quarantine.

1. Quarantine of observation consists in keeping separately and under surveillance a ship, its passengers and crew, for a period of some days from the time of the admission on board

of the health guardians, the time to be regulated by the local sanitary authorities. It does not require the disembarkation of the passengers, nor the discharge of goods or merchandise, unless they be injured or in decomposition. It enjoins free ventilation of the ship and general measures of hygiene, but not its disinfection. It may be gone through at any port in which a sanitary establishment exists. In some special cases the passengers may be disembarked and landed at the lazaretto. It is applied to ships in good sanitary conditions, which have not suffered from cholera.

2. Strict quarantine is the isolation for a fixed time of the ship and persons, with disinfection of all that may contain the seeds of the disease. It demands the discharge of all the merchandise into the disinfecting houses, the disinfection of certain articles or goods, and the landing of passengers at a lazaretto. It is applied, 1, to ships from an infected port with a foul bill of health, with certain exceptions; 2, to ships which have had cases of cholera on board during the voyage, although they may have a clean bill of health. Strict quarantine begins with ships in ballast when the quarantine officers commence their watch on board; for other ships, after landing of the goods; for persons, immediately that they enter the lazaretto.

Thus, according to the Conference, quarantine of observation is a term of probation, of simple watching; while strict quarantine consists in the landing of passengers at a lazaretto, with the use of disinfection, and of every measure of precaution that can be applied to arrivals from infected ports.

Time of separation of suspected persons.—The Conference has recommended by a majority the period of ten days as the time of separation or isolation of arrivals from infected places both for land and sea quarantines. This term has been fixed upon because, while it seems likely to accomplish all that we expected from quarantine, it will not be so onerous to the interests of commerce as the periods of sequestration adopted by some nations during the present epidemic. The Conference in this matter has endeavoured to regard both the demands of commerce and the interests of science.

No part of its task has been more difficult than the fixing of the term of probation. The period of incubation of cholera has not been anywhere determined with accuracy. On the one hand, it may be very short, probably less than one or two days from the reception of infection, as in cases of confirmed cholera occurring rapidly after exposure. On

the other hand, we cannot tell exactly how long a time may pass after exposure before symptoms of diarrhœa begin to appear, or how long a diarrhœa may exist or be concealed before it passes into unmistakable and characteristic symptoms of choleraic disease. No quarantines can be efficient unless the element of diarrhœa is taken into account, as it is probable that persons with this affection are very active agents in propagating the disease, and more dangerous than others to the community, because they wander about unsuspected for some days after the commencement of their symptoms. Still it appeared to the majority of the Conference that in the larger number of cases a true choleraic diarrhœa would in the course of eight days have pronounced itself sufficiently to have prevented concealment of its existence. It was thought, therefore, that in fixing ten days as a period of sequestration, sufficient time would have passed to permit of the discovery of disease. Were the existence of all cases of diarrhœa discoverable, the difficulty would be much diminished. It is probable that much less than ten days would be required after leaving an infected place for the declaration of confirmed cholera or of the commencement of diarrhœa. As this cannot always be discovered, it was thought necessary to allow sufficient time for the disease to betray itself. The new plans of quarantine differ from the old in recognising the vast importance of cases of choleraic diarrhœa, and in endeavouring to act against them as much as against cases of confirmed cholera. The Conference was well aware that a much longer period than this has been assigned to the duration of choleraic and infectious diarrhœa; but it must be borne in mind that all cases of diarrhœa occuring in cholera epidemic are not choleraic diarrhœa, and that many of the prolonged cases are not really choleraic. It may also happen that an exaggerated duration may have been given to cases which does not belong to them. Some of these may have commenced as common diarrhœa, the subjects of which were afterwards attacked with choleraic or specific diarrhœa. At all events, to have taken the four, five, or six weeks, which tables assign as the duration of some cases of diarrhœa occurring in cholera epidemics, as a rule for guidance in framing measures, would require an amount of quarantine which no people would or could tolerate in the present days of rapid communication. The Conference has therefore adopted the conclusion "that strict quarantine applied to persons coming from an infected place be fixed at ten full days as a general rule, and that this

quarantine should commence for all persons from the moment of their entry into the lazaretto. If, however, during the course of the quarantine cases of cholera or choleraic diarrhœa appeared, the healthy persons should, after separation of the sick, recommence the quarantine of ten full days."

The Conference further voted that there is reason to consider as suspicious all persons affected with diarrhœa, "to separate them from the healthy in the same manner as cases of cholera, and not to afford them free pratique at the end of the regulated period of quarantine until medical inspection has declared the non-choleraic nature of the diarrhœa." By thus submitting all cases of known diarrhœa to this severe trial, and throwing the responsibility of permitting them to end the period of quarantine upon the sanitary officer of the port, the risk attendant upon the escape of prolonged cases of choleraic diarrhœa will probably be corrected, and the necessity of quarantine based upon an unusual duration of choleraic diarrhœa be obviated.

The Conference has considered the question as to whether the time of the passage may in any case be reckoned as part of the quarantine, and it has decided that in certain circumstances it may be safely allowed. The circumstances which will permit a ship sailing from an infected port to have this advantage are—first, the presence of a surgeon on board appointed to the duty; and secondly, the submission to a series of precautionary measures at the port of departure, during the passage, and at the port of arrival. The measures at the port of departure will be mentioned further on, the object being to insure a clean ship, inspection of the persons embarked, and absence of crowding. During the passage, measures of disinfection, free ventilation, and cleansing are required. All this is to be done under the inspection of the medical officer, who will keep a register of sickness occurring on board, and submit it to the sanitary officers at the port of arrival. Under these conditions, and with the absence of cholera or choleraic diarrhœa during the passage, the Conference is of opinion that the time of the voyage may be taken as part of the quarantine, and the ship will be submitted to twenty-four hours' surveillance only at the port of arrival. Most of the regular passenger ships meet the above conditions, or can make arrangements to comply with them without great difficulty. It will be obvious that ships under these conditions making long voyages will suffer but little

delay, but the reverse is the case when the voyages are short. The scale of quarantine runs as follows :—

A voyage of	24 hours	..	9 days'	of observation.
,,	2 days	..	8 ,,	,,
,,	3 ,,	..	7 ,,	,,
,,	4 ,,	..	6 ,,	,,
,,	5 ,,	..	5 ,,	,,
,,	6 ,,	..	4 ,,	,,
,,	7 ,,	..	3 ,,	,,
,,	8 ,,	..	2 ,,	,,
,,	9 ,,	..	1 ,,	,,

For those beyond nine days, twenty-four hours of observation should always be required.

Merchant ships not complying with the above conditions, which have passed fifteen days on the voyage, and without cases of cholera on board, are allowed pratique after five days of quarantine of observation. For vessels in bad sanitary conditions, such as pilgrim ships with crowding of people, and with cases of cholera or choleraic diarrhœa, the severest quarantine measures are to be adopted, and the period of sequestration may be prolonged if the sanitary authorities of the port of arrival think it necessary.

These quarantine measures are calculated to encourage good hygienic arrangements in ships, because they favour all such as are in good conditions of health, and are very severe upon those which neglect precautions. They make the condition of the ship the guide to the mildness or severity of their application.

Lazarettos have occupied much of the attention of the Conference. It will be impossible to mention in detail all the arrangements proposed. They are to be placed in situations such as islands, uninhabited spots, and where they cannot serve as centres of infection to the countries in which they are placed. By a majority the Conference recommend that, where geographical circumstances permit, lazarettos common to two or more nations would be desirable.

The plans of the lazarettos provide for separate pavilions, with ample space for accommodation, and means for separating different batches of arrivals; and also hospitals for separating the sick from the healthy. The lazarettos are to be made as comfortable as possible, well provided with good water and provisions, to have sufficient and good medical attendance, and to be submitted to efficient inspection by superior authority. Should these recommendations be carried out, lazarettos will cease to be the wretched dens of

the past, and indeed, in many instances, of the present times.

Much of the discomfort of the lazaretto, however, will be obviated by the recommendation that ships in certain conditions shall be submitted to quarantine of observation only, which does not require disembarkation.

On the whole, long as the period of ten days may seem to English minds, it would be difficult to devise quarantine systems with less inconveniences than those above mentioned, if security and efficiency are to be expected of them, and if they are to be adopted at all. They are certainly feasible in ordinary cases. They would probably be of little value in narrow seas, such as separate Great Britain from the continent, unless *all* vessels arriving from all ports of the opposite coasts be quarantined when the disease exists in one or two ports only. It would not suffice to place the infected port merely in quarantine. This system would be so onerous that it would, doubtless, meet with great opposition; but in cases in which a ship would have to perform a voyage of two or three days or more from port to port, as in our own relations with Spain, Portugal, and the Mediterranean, it would be well worthy of adoption, and would, doubtless, check the introduction of cholera into the countries so placed: and this is the main point to be considered.

The Conference has thought it would be an advantage to throw obstacles in the way of the exportation of cholera from an infected port, as well as to check its importation at the port of arrival, and has recommended a series of measures, which are embodied in the "note additionelle" to the measures of hygiene, the chief object of which is to submit to medical inspection all passengers embarking at the infected port, as well as the crews of the ships; to forbid the embarkation of all cases of cholera or of diarrhœa, except cases of chronic diarrhœa or dysentery provided with medical certificate; to ensure the goodness of food and water, the cleanliness of linen and clothes, and to prohibit the embarkation of all merchandise susceptible of contamination, such as articles of clothing, rags, and skins.

The Conference has considered the application of many of the foregoing recommendations to cases of towns threatened or attacked by cholera epidemics. During an invasion daily medical visits should be made to the houses of the poorer classes, to ascertain their condition, and to afford immediate assistance if necessary. Popular and intelligible instructions should be issued, explaining the sources of danger and modes

of propagation of the disease. The use of common privies should be forbidden, and the disinfection of cholera excreta rigorously carried out, as already mentioned. Separate hospitals, conveniently situated, should be provided; but in the event of this not having been done, and admission into existing hospitals being imperative (and, where possible, this is always to be avoided), patients should be placed in separate wards and isolated. The cholera sick should be conveyed in carriages specially destined to the work, and used for no other purpose; the evacuations disinfected; the soiled and other linen in use by patients placed immediately after use into solutions of chloride of zinc; bedding, thick clothes, and all such as cannot be safely disinfected, destroyed by fire. The nurses to be chosen, if possible, from persons who have already had cholera; that they have frequent reliefs of duty, and that their hours of rest be passed out of hospital or the sick-room. The dead of cholera should be wrapped up in the bed-clothes in which they die, and placed thus in the coffins, without the usual laying out of corpses; the coffin to be filled up with quick-lime. Houses of refuge to be provided for removal of families from the contaminated houses.

Upon the question of the removal of people out of infected or threatened towns, the Conference thinks that it may be safely done before the epidemic breaks out. The diminution of over-crowding would be useful, and the emigration of the people is not dangerous to the places to which they fly. The case is otherwise when the epidemic exists, but this should not prevent the dissemination of people over uninhabited neighbouring parts, or the camping out on waste places, when practicable. In this manner—by thinning of populations—the ravages of an epidemic may be lessened without risk to surrounding places. Encampments, however, should be submitted to the strictest rules of hygiene, and be provided with pure drinking-water. It is recommended that the authorities of places threatened with a cholera epidemic should see to the thinning out or redistribution of the inmates, of all institutions or dwellings, among available buildings, where over-crowding exists. Fairs and congregations of people should be stopped, and the movements of troops suspended, when possible.

The Conference has judged that, India being the place of origin of cholera, it would be of great moment to attack the disease in India itself, to prevent its exportation therefrom, and to check its progress westward by restrictive measures;

believing that these will be all the more efficacious the nearer they are applied to the sources of the disease. Cholera may reach Europe, by sea, through the Persian Gulf, and by the Red Sea; by land, through Central Asia, Persia, Syria, and Russia.

It would be very desirable that we should know something accurate of the mode of origin of cholera in India, and this would probably assist us to extinguish the disease in its cradle, or in parts of India in which it is endemic. It is very possible that the endemic centres are the starting-points of the different Indian epidemics, and that as regards these, the endemic centres are to the greater part of India much as India is to the rest of the world. If we could extinguish cholera in the localities in which it is permanent, the Indian epidemics would perhaps not arise. This important point in the etiology of cholera, like that of many other diseases, is unknown. The manifest advantage of such knowledge has induced the Conference to appeal to the British Government to institute inquiries into the etiology of the disease, in the hope of thus obtaining information of great value in prophylaxis. Upon the question of the origin of cholera in places in which it is endemic or permanent, I wish to express my great doubts whether the disease has any local terrestrial origin; that there is any choleraic miasm proceeding from the soil as a natural product. It appears to me and to my able colleague in the Commission, Dr. Dickson, that the fact of cholera being a transmissible disease is sufficient to account for its permanence in certain favouring situations. A transmissible disease in conditions favourable to its multiplication—*i. e.*, in bad sanitary conditions—may maintain itself permanently in a locality. If this is the case we may dispense with ideas of spontaneous generation of the disease, or with the somewhat favourite theory of a miasm issuing from the soil of the Delta of the Ganges. The mode in which cholera has maintained itself permanently in places beyond the Delta of the Ganges, as in Bombay for instance, from which it has not been absent a single month during the last twenty years, shows its capability of assuming an endemic form in situations which can hardly be called its birth-place; and if it can do so in Bombay, why not in the towns in the valley of the Ganges, where bad sanitary conditions are rife enough, and where favouring meteorological conditions may also be found? It may be, then, that what may be called the endemicity of cholera is little more than a prolonged epidemic. It would be a fortunate thing for

humanity if researches could establish this. It would be far more easy to extinguish the disease by removing the man-created favouring and remediable conditions of multiplication than it would be to alter the physical conditions of the soil of the Delta of the Ganges. The first is possible with well-directed and continuous effort; the latter may be fairly considered impracticable.

The Conference thinks that the congress of Hindoo pilgrims at the numerous shrines and fairs readily accounts for much of the propagation and diffusion of cholera in India; and it recommends the general adoption of measures of hygiene, and of restriction upon the pilgrims returning home, already in use in some localities. It also asks the Government of India to continue in the path of sanitary reform which it has already energetically commenced, and looks forward to great advantages therefrom, in diminishing if not extinguishing cholera. It submits that measures of restriction, where practicable, might, pending the full realisation of sanitary measures, assist in checking the propagation of the disease.

It is important, as regards Europe, to check the exportation of cholera from India. This is proposed to be done by raising barriers by land and sea. It is supposed by the Conference that it might be checked on the Punjab frontier by the Indian Government. I hardly think that this would be possible of attainment. There would be more probability of restraining its escape by the sea-board, by the adoption of the rules I have already mentioned as applicable to the ports of departure for ordinary vessels, and of stringent regulations for the ships which convey the Mahommedan pilgrims to Jeddah. The sea routes by which cholera may pass towards Europe are the Red Sea and the Persian Gulf. The Conference has left the Persian Gulf route to the ordinary rules of quarantine, but has, in addition to them, recommended special measures for the Red Sea route, chiefly on account of the Mahommedan pilgrims. Time does not permit me to do more than enumerate them. For the ordinary communications we have the quarantine regulations already mentioned. The Indian passenger ships to be inspected at Perim, and made to perform quarantine at Tor if necessary, or to receive pratique at Suez, which they would get after twenty-four hours, as the voyage would be allowed to count as part of their probation. The pilgrim ships, on leaving India, to be subject to the Native Passengers Act of 1858 in the case of all ships and of all flags, and to the rules

embodied in the recommendations of the Conference for the regulation of vessels leaving an infected port. On entering the Red Sea, to be inspected at Perim, and to perform quarantine, if necessary, at some lazaretto, the site of which has not been settled. At Mecca the same sort of scavengering, burial of excreta and all organic refuse, are always to be carried out, as they were for the first time this year. In the event of cholera breaking out in the Mahommedan Holy Land, the pilgrims may return westward by caravans if they please, in the journeys of which the disease would probably die out; or, if by sea, they must perform fifteen days' quarantine at El Wesch, in the northern part of the Hedjaz. Sanitary establishments are to be erected at Koseir, Souakim, Mussoweah, at Jedda, and at Yembo, in communication with and under the control of an international sanitary body sitting at Suez. If, in spite of this, cholera should be carried to Egypt in any way, it was advised by the Conference that all communication between Egypt and the Mediterranean ports should be cut off during the course of the epidemic. Mails only, however, might be allowed to pass.

Such is a bare outline of the measures proposed for the Red Sea route. I cannot help thinking that the inspection at Perim would be less valuable than the Conference supposes. It would certainly stop the large ships; but the danger to the Hedjaz is not only from the large ships sailing direct from India, or, I venture to think, chiefly from them, but also from the small craft which communicate between the Arabian coast and the ports of the Red Sea, and which would escape observation. The lazaretto at the entrance of the strait present many difficulties in providing supplies, anchorage, and defence against hostile tribes, and would not dispense with internal quarantine stations. It becomes, therefore, a question whether it might not be less troublesome to do away with the inspection at Perim, even if the British Government would permit the use of the island for the purpose of a station, and let the vessels clear out from Indian ports to recognised ports in the Red Sea at which quarantine stations might be instituted. The idea of arresting all vessels at the entrance of the Red Sea and thus baring out cholera, is a good one in theory; but I think it questionable whether it can be made efficacious. The measures taken in the Hedjaz, and the quarantine applied to the western pilgrims at El Wesch, if faithfully carried out, ought to be very useful, and seems likely to check the progress of an epidemic westwards if it should again occur at

Mecca. The British Commissioners voted against the radical measure of interruption of all communication with Egypt in the event of cholera being there. I would observe that I have not been able to discover good evidence, much as has been said about it, that cholera was imported into the Hedjaz direct from India last year. The two ships, *Persia* and *North Wind*, on board of which so much mortality occurred, had no choleraic disease when they left Singapore. The captains both state that the passengers and crews caught the disease at Mokulla, on the Arabian coast, and that it raged severely among them until they were opposite Leet, about one hundred miles below Jeddah. It appears, therefore, very probable that, in spite of all rumours to the contrary, the disease was not imported into the Hedjaz directly from India in 1865. It may, however, have been imported previously from India into some part of the Persian Gulf or Arabia. Unfortunately we are not sufficiently acquainted with the condition of Arabia to know to what degree, or how frequently, cholera appears there. The Conference has suggested the advisability of measures being taken along the Turco-Persian, the Russo-Persian, and the Russian frontiers in Central Asia. It is true that in some of these long lines certain passes only are available for communication, and that the line to be guarded is less troublesome than might be expected; but the people to guard them are not much to be depended upon. Too much must not be expected from these suggestions. If Persia would rouse herself to assist in checking the disease by the sanitary measures suggested by the Conference, as well as by general attention to questions of public health, valuable assistance might be afforded in arresting the advance of cholera in its progress westwards. Owing to the frequency with which cholera has appeared in Persia of late—as, for instance, in the course of eleven years, 1851-62, there were cholera epidemics in 1851, '52, '53, '54, '55, '56, '57, '58, '60, and '61—she must be considered as dangerous to Europe, because cholera almost threatens to become acclimatised there. Should cholera unfortunately again come westwards through the northern route, I fear that it will not be checked through Persia or Turkey in Asia, but that we shall have chiefly to depend on such measures as may be taken in Russia, on its frontiers, and especially on the Caspian Sea, and upon restrictions in the Black and Mediterranean Seas. Should it come again by the Southern route, I think that more success may be expected. The measures to be taken in the Hedjaz and the long quarantine

or sequestration at El Wesch ought, if fairly carried out, to prevent the extension beyond the Hedjaz, and prove an effectual barrier to its advance to Europe. Failing these measures of prevention, the preservation of Europe must depend upon the general means of hygiene, purification, and restriction, pointed out as applicable in all countries.

I have not been able in the limits of this paper to enter into all the details of the discussion, reports, and measures proposed by the Internatianal Sanitary Conference; but I have endeavoured to show you the verdict which it has given on the main point before it. I will, in conclusion, observe that the advice of the Conference is not to trust to sanitary measures alone, or to restrictive measures alone, but to rely on the union of these with measures of disinfection. It is probable that neither hygiene nor restriction of intercourse will be so perfect in their execution as to suffice singly to combat the disease; but in the combination of both we may hope that the shortcomings of the one may be remedied by the workings of the other.

ON YELLOW FEVER IN RELATION TO THE HOME POPULATION.

By GAVIN MILROY, M.D.,

VICE-PRESIDENT OF THE SOCIETY.

[*Read February 4th*, 1867.]

As within the last eighteen months this foreign pestilence has been twice at our very doors, first in the remarkable outbreak of several fatal cases on shore at Swansea, in September 1865, and again on board five or six of the West India steamers which have arrived at Southampton since November last, it clearly behoves us, as the Society which specially professes to deal with such matters, carefully to investigate the subject, so that at least we may be prepared to form a reasonable opinion on some of those important questions of State Medicine connected with it, and the full and impartial examination of which the interests of the public welfare, as well as of medical science, obviously require. The simple object of the present communication is to bring before you the principal data which the experience of the last twenty years has served to furnish, in relation to the proper measures for the defence of our own shores against the introduction and spread of this so much dreaded disease. The paper, it will be seen, is strictly narrative and suggestive, and not controversial, my only wish being to induce the Society to have the subject thoroughly investigated as it deserves to be.

H.M.S. *Eclair*, a paddle steamer, with a crew originally of one hundred and forty Europeans, exclusive of a good many African sailors or Kroomen on board, suffered severely from yellow fever on the West African coast in the summer of 1845. When she arrived at the Motherbank on the 28th of September, upwards of sixty of her crew had perished, and there were then twenty-three on the sick list, of whom five were ill in bed, and the rest were in different stages of convalescence. Since leaving Madeira, where pratique had been refused, seven days before, four deaths and eight new cases had occurred. Mr. Bernard, the acting

surgeon on board, in his report on the arrival of the ship, remarked:—"There exists among all aboard, officers and men, the sick as well as the healthy, a degree of mental depression that is exceedingly distressing and injurious. I am, therefore, very apprehensive of the disease increasing, and would beg to represent for your consideration whether a removal from ship-board would not materially assist in preventing the further progress of the fever."

Rear-Admiral Parker, the naval commanding officer at Portsmouth, wished to send at once a hulk for the removal of the healthy crew from the pest-smitten vessel; and, on consulting with Sir John Richardson, then the physician of Haslar Hospital, the latter recommended the immediate removal of the sick and the convalescent to a wing of the hospital, observing that "notwithstanding the extraordinary mortality that has swept off so large a proportion of the crew, I entertain no fears of her being the means of introducing epidemic disease into this country; and were the sick placed in well-ventilated wards, with fresh bedding and the other means of cleanliness afforded by a hospital, I anticipate no further risk to the attendants than would occur in wards set apart for cases of typhus fever."

Sir W. Pym, then the medical superintendent of Quarantine, thought otherwise; and the result was that the *Eclair* was detained in strict isolation at the Motherbank (even letters were not allowed to be sent on shore), until the evening of the 1st October, when she was ordered round to Standgate Creek in the Medway; the reason alleged for this procedure being "the difficulty of communicating with the vessel at the Motherbank in the event of boisterous weather coming on," while at Standgate Creek "she would have smooth water, and the assistance of the regular quarantine station."*

She arrived there next day, and, on the 3rd, the unattacked portion of the crew were transferred to the two-decker the *Revenge*; the convalescents, and those who had recovered, to the *Benbow*; while the sick, the nurses, medical officers, and Africans were kept in the *Eclair*. Five deaths had occurred on board the ill-fated ship since her arrival in England; three of them on the day after reaching the Medway. On the evening of that day Mr. Bernard was attacked;

* The allusion to the unsuitableness of the Motherbank, as the quarantine station in boisterous weather, is to be noted in connection with the recent detention there of the infected West India steamers during winter.

and two days later, Mr. Coffey, the assistant-surgeon, fell sick, but his seizure was slight. Mr. Bernard died on the 9th. Two of those also, who had been transferred to the *Revenge*, sickened with the fever three or four days subsequently; they were taken back to the *Eclair*, on board of which they died; the one on the 11th, and the other, the last victim of the scourge, on the 12th October, or fourteen days after the arrival of the ship at the Motherbank. No fresh case occurred afterwards; and by the 15th all the sick were reported to be convalescent. On the 20th and 21st, indeed, two men, who had acted as attendants upon the sick, and afterwards superintended the clearing out of the *Eclair's* hold by the African sailors (none of whom had suffered at any time), were seized with feverishness and irritability of the stomach; but these symptoms speedily subsided. In connection, however, with the temporary sickness of these men, it is well worthy of notice that, about this very time, two of the crew of H.M.S. *Growler*, which had also arrived from the African coast at the same time as the *Eclair*, were attacked with fever while the vessel was lying off Woolwich. They had been engaged in clearing out the hold. They were taken to the marine hospital on shore, and died with all the symptons of yellow fever. No other case, I believe, occurred, either among her crew, or in the hospital.

In concluding the notice of the case of the *Eclair*, it is to be noted that all the persons attacked with the fever after her arrival in this country were, or had been, on board of the infected ship. "The disease was not communicated to a single person out of the vessel;" and with respect to the delay that took place in removing the crew immediately upon arrival in England, Dr. Bryson has remarked—and his opinion was shared by the then Director-General of the Navy, the late Sir W. Burnett—that "The men who had hitherto escaped, being in a state of great alarm, were prone to take the disease which, by being removed, they might have escaped; while the sick, had they been taken to a wing of Haslar Hospital, would have recovered more rapidly. Or, had the vessel been taken into Stokes' Bay, the sick and healthy landed, and placed in an airy, elevated, and dry spot, under canvas, there is every reason to believe that the subsequent loss would not have been so great; and the despondency, which weighed the men down, would have given place to hope and cheerfulness."*

* An Account of the Origin, Spread, and Decline of the Epidemic Fevers of Sierra Leone, etc., 1849.

From November, 1852, to the following May, four of the West India mail steamers—the *La Plata, Medway, Orinoco*, and *Parana*—arrived at Southampton from the West Indies, infected with, or suspected of having, yellow fever on board. In all of them, deaths from the disease had occurred during the voyage from St. Thomas, where the fever was then prevailing. The total number of cases in the four vessels was 98, and of deaths, 22. The number of sick on arrival amounted to 12. The crew in each ship was 122, officers and men. The total number of passengers amounted to 330, giving an average of 82 or 83 to each vessel. The dates of arrival were 18th Nov., and 19th Dec., 1852, and 18th January, and 30th May, 1853. The duration of the quarantine imposed upon these four vessels varied much; in one case it was only eight hours, in another about two days, in a third three days, and in a fourth nearly four days. None of them, to the best of my memory, were sent to the Motherbank; but on this point I may be mistaken. As far as the very imperfect records that have ever been published respecting these interesting occurrences shew, only one fresh case of sickness took place among the 818 persons who landed in Southampton from these four steamers. It is certainly very much to be regretted on the ground alike of professional knowledge and of public enlightenment that, notwithstanding repeated appeals in the medical journals at the time to the medical officers of the infected vessels, and to the quarantine authorities at Southampton, no detailed information has ever, as far as I know, been given to the public respecting the several events, save in regard to the first arrival, that of the *La Plata*. If equally reliable and full accounts of the other vessels were before us, it cannot be doubted but that some useful information might have been added to our scanty stock. Nothing has hitherto stood so much in the way of arriving at sound and reasonable conclusions on the subject of quarantine, as a means for the exclusion of apprehended diseases, as the dearth of authentic and complete information about details; it is the 'logic of facts' that is most wanted; and assuredly this Society would do no small public good if, by its representations, it brought about the much needed reform in the taciturnity-system that has hitherto prevailed. With respect to the *La Plata*, it appears that she had 14 cases of yellow fever, 7 of them fatal, during the voyage. The last death occurred on the 17th, the day before her arrival at Southampton. None of the passengers, 57 in number, had at any time suffered. It appears that they were

permitted to land on the 19th, and the vessel herself received pratique next day. There were four convalescents on board at the time. The only casualty which occurred afterwards was that one of the engineer officers, who continued to perform daily his duties in the hold after the admission of the ship into port, began to sicken on the eighth day after her release—the tenth after her arrival—and died on shore on the sixth or seventh day of attack, with most of the characteristic symptoms of yellow fever. None of the attendants nor any inmate of the house was at all affected. With respect to the other vessels, all that we know is that no fresh attack of sickness occurred in any of them after arrival, and, moreover, that in all of them, as well as the *La Plata*, the passengers had throughout the voyage remained in perfect health, although the medical officers, and some of the other occasional attendants upon the sick, were in free intercourse all the while with every one on board, in the saloon as well as in the steerage cabins.

The following highly interesting details are taken from a paper communicated to this Society by Dr. Slayter, then health officer of the port of Halifax, and printed in Vol. I. of our *Transactions*.

"In the summer of 1861, a very sickly yellow fever year, seven ships of war of our West India squadron arrived at Halifax, with cases of the fever on board; some of them had suffered very disastrously during the voyage from the West Indies. Halifax is between five and six degrees of latitude to the south of Southampton; its climate, however, is very much like that of England. The first ship that arrived was the *Firebrand* on the 8th of July, when the thermometer was generally about 76 deg. or 78 deg. during the day; at night it ranged from 55 deg. to 59 deg. She had lost 39 out of her crew of about 170, (ten of these having died on the voyage from Jamaica), and there were 79 on the sick list, 34 being seriously ill, and the rest in different degrees of convalescence. The former, and some of the convalescents were at once transferred to the *Pyramus* hulk, lying opposite the naval hospital in H.M. dockyard; the rest of the convalescents and the healthy (with the exception of those who remained on board the *Firebrand* to cleanse and ventilate her) were accommodated in a storehouse in the dockyard, and the officers occupied apartments in the commissioner's house adjacent to the storehouse. This was considered the only quarantine regulation necessary. One of the engineers lodged in the commissioner's house sickened forty hours

after landing; he was removed to the *Pyramus*, and died soon afterwards with black vomit. Besides this fresh case, one of the men who had been left in charge of the *Firebrand*, and who had been taken to the naval hospital, died there of the fever a fortnight after the arrival of the ship at Halifax; while another of the crew who had been acting as nurse in the *Pyramus*, and who had been taken to the hospital, recovered. Altogether, there were in all 84 cases treated in this city, 16 died in the *Pyramus*, and 2 on shore in the hospital."

The case of H.M.S. *Spiteful*, which arrived from the Bahamas in the following month, was in most respects similar. She had lost 24 of her crew (11 having died in the voyage from Nassau), and there were 46 on the sick list. The measures adopted were the same as what had been resorted to with the *Firebrand*. Several fresh cases occurred among the men who had been landed; they were sent to the *Pyramus*. Three of the fresh cases were remarkable from the long interval, of 14 or 15 days, which elapsed between their occurrence and the arrival of the ship at Halifax. Two of them occurred in men who had been left on board of the *Spiteful*, and the remaining case in a boy who had had frequent communication with the infected ship. Altogether, the number treated at Halifax was 33, of whom 12 died.

After narrating briefly the history of the other sickly ships of war which arrived that season from the West Indies, Dr. Slayter remarked—"I am happy to state that none of the people about the dockyard or in the city took the disease;" and he adds—"On account of the healthy state of our climate, our local government has hitherto considered it an unnecessary expenditure for the erection of a quarantine hospital. The subject, however, will be brought before them at an early period; and the past summer's experience teaches the necessity for a more vigilant attention on the part of the naval authorities, and also the want of a suitable building wherein to transfer the sick on their arrival in this port."*

Since Dr. Slayter's death, our Society has unfortunately had no other communication on the subject from Halifax. Similar details respecting the sickly ships of war, or others, which have arrived there from the West Indies in subsequent years, would have been of the greatest value.

I must now ask you to cross over with me from Nova

* Transactions of the Epidemiological Society, vol. i, p. 186.

Scotia to the coast of Brittany, in France, in latitude intermediate between that of Halifax and that of Southampton. The year of the occurrence to be related was the same, 1861.

The *Ave Maria*, a sugar ship, arrived at St. Nazaire, at the mouth of the Loire, on the 25th July 1861. She had sailed on the 13th June from Havana, where she had lain for upwards of a month taking in her cargo; that port was then infected with yellow fever. None of her crew, 16 in number, had been at all seriously ill while there. The earliest two cases of fever on board occurred seventeen days after leaving Havana; both proved fatal within four or five days. Seven others of the crew, including the captain, sickened with the fever; but they all recovered, and were more or less decidedly convalescent on arrival at St. Nazaire. Conformably to the then existing quarantine regulations in France, which granted pratique to all vessels in which no death or fresh attack of the fever had taken place within ten days before arrival, the ship was at once admitted into a wet dock along the least frequented quay in the port. The crew, as is usual in France, at once left the vessel, and dispersed to their homes in different directions, while seventeen labourers, all in perfect health, were engaged to discharge the cargo. This occupied eight days in all. The former, viz., the crew, appear to have remained quite well; the convalescents recovering their health, and the others continuing free from any illness. Very different was the fate of the labourers; two-thirds of them were affected, and many of them died with the characteristic symptoms of yellow fever. Nor was the sickness confined to these men; for numerous cases occurred among the crews of other vessels lying close alongside, or having direct communication with, the *Ave Maria*. In the great majority of instances, the sufferers had been on board of the infected ship. Altogether, 40 men were attacked with the fever, and of that number 23 died. Many of the cases occurred in the town, and several in the immediate neighbourhood of the ship. Most of them were treated in their own homes; a few only, who were among the more destitute, were removed to a temporary hospital or lazaret.

With respect to the state of the *Ave Maria*, it appears that not only was her hold abominably foul and offensive, but there was also a large accumulation of decaying rubbish, with dead rats, old bones, and different odds and ends in between the side timbers of the ship, so that altogether she was as thoroughly unwholesome as she well could be. She was, of course, removed from the dock into which she had

been admitted to the opposite side of the Loire, where she was kept apart, and subjected to a most thorough purification and disinfection.

Soon after the case of the *Ave Maria*, several other merchant vessels from the West Indies arrived at St. Nazaire, having suffered from the fever on the voyage, and in some instances with cases of the disease on board on arrival. None of them were admitted into the port or harbour, but were stationed on the other bank of the river, where they could not compromise the rest of the shipping. The passengers, and these of the crews who could be spared, were removed at once on board a hulk appointed for the purpose, where each had a bath, and was required to change his linen and clothes. Those who were actually sick were transferred to a hospital ship; and, with respect to the discharging of the cargoes, this was carried out with certain hygienic precautions which constitute what M. Melier, the chief of the quarantine department in France, calls *dechargement sanitaire*. By these measures all the infected vessels were unloaded without any casualty, save in a single instance, where one of the labourers engaged in the work, and who had wilfully neglected the regulations by remaining much longer in the hold of one of the vessels than he ought to have done, was attacked with fever at his own house on shore; he was subsequently taken to the hospital ship, and died there on the seventh or eighth day after the seizure. Neither in this case, nor, as far as we know, in any of the other similar examples of men who sickened in their own homes, did the other inmates of the dwellings suffer from the disease.

It is to be observed that none of the men employed on shore in handling or conveying the boxes of sugar from the wharf to the railroad, even before any precautions were resorted to in referencc to this part of the unloading of the cargoes, were at all affected at any time.

In consequence of these occurrences at St. Nazaire in the summer and autumn of 1861, and the elaborate report upon them by M. Mélier in the following year, the French Government deemed it right to introduce considerable modifications into their quarantine code in reference to vessels suspected of, or infected with, yellow fever. That code had been based on the recommendations of the International Sanitary Conference held in Paris in 1851. Without entering into particulars as to details, it may be sufficient at present to state that the duration of the quarantine detention of

healthy persons was materially shortened, while that upon the vessels themselves was made to depend much upon their sanitary condition, whether good or otherwise. When cases of the fever had occurred in vessels whose sanitary arrangements were thoroughly good, a detention of from three to seven days was imposed on the passengers on board; but this period might be much shortened, or altogether dispensed with, under peculiar circumstances when the Government saw fit, upon the report of the sanitary officers. The discretionary power of the Minister of Commerce was in all cases to be supreme.

In conclusion, it deserves to be stated that, of recent years, the arrival of ships in the oceanic ports of France with yellow fever on board, or which have suffered from it during the voyage, is of much more frequent occurrence than it used to be. Few years now pass over without instances of the sort in one or other of the harbours of that country. This tendency to the greater extension of the disease of late years has been, it will be remembered, brought before the notice of this Society on several occasions.

In 1862, a trading steamer, with a cargo of cotton and having a crew of 50 men and 23 passengers on board, evading the quarantine regulations at Gravesend, arrived at the Victoria Docks in the river on the 20th of August. She had left Nassau in the Bahamas on the 2nd of the month, having previously, it was believed, run the blockade at Charleston. Seven or eight deaths had occurred during the voyage, the latest only three or four days before reaching the Thames, the patient having sickened about ten days previously. The captain and two of the passengers had landed at Dover. On the facts becoming known, the vessel was ordered to be pushed off into the middle of the basin, apart from the other shipping, and a strict guard kept over her to prevent any communication with the shore. The pilot, two or three of the custom-house men, and some other people who had gone on board, were not permitted to land. After three days' detention, the vessel being ascertained on inspection to be remarkably clean, and the clothes and bedding of the persons deceased having been burned, pratique was allowed her. No fresh casualties of any sort occurred; and no further notice was taken of the case.

The *Hecla*, a sailing vessel, arrived at Swansea on Sept. 9, 1865, with a cargo of copper ore from St. Jago de Cuba, which she left on 26th July. She was provided with a clean bill of health; although, as it afterwards appeared,

yellow fever was prevalent there at the time, and one or two of the crew had died while the vessel lay there. On being boarded by a pilot in the Bristol Channel, twenty-five miles or so from Swansea, he was told that one man on board was ill with dropsy, and that, three of the crew having died on the voyage, the ship was short-handed, and required four or five men from the shore to bring her into port. Five hands were accordingly sent on board; and next day, not a word having been said as to the cause of the deaths among the crew, she was brought in without any quarantine flag hoisted, and placed in one of the docks alongside the usual discharging quay. A good many persons went on board at once, two passengers landed with their luggage, and the crew distributed themselves in the town. Three men were landed sick; two of whom were convalescent from "fever," one (the captain), reported ill of dropsy. Within three hours of the arrival in port, the hatches were removed, a stage rigged, and gangs of men commenced discharging the cargo. It soon became known that the deaths on board during the voyage had been from yellow fever, and that one at least of the sick men who had been landed, and taken to a wretched room in a small dirty house, was actually moribund from this disease. He died in the course of the day. The discharging of the cargo was immediately stopped; and no person was permitted to go on board, except those employed in purifying and disinfecting the vessel, which was foul-smelling in different parts, by washing the deck and forecastle with a solution of chloride of lime, and by causing the free evolution of chlorine in the forecastle, steerage, hold, &c., &c. After these measures had been used for three days, the discharge of the cargo was recommenced, and it was completed in eight or nine days, or by the 21st or 22nd September.

The earliest case of sickness connected with the *Hecla* after her arrival seems to have occurred, on Sept. 15th, in a ship-rigger, who was at work in a vessel lying immediately astern, and who had been on board of her several times. He recovered, and no other inmate of his house suffered.

The first fatal case occurred two days later. The man, a Custom-house officer, whose duty it was to patrol the quay immediately opposite to where the *Hecla* lay, and who had also been on board of her, sickened on the 17th, and died on the 22nd. No other person in his house was affected. Other cases rapidly followed during the next fortnight; so that altogether, "between Sept. 15th when the first case

occurred, to Oct. 13th when the last case was convalescent, 22 cases in which the diagnosis of yellow fever could pretty certainly be made, and 7 other cases in which the circumstances of exposure, or the character of the attack, led to a more or less strong suspicion that the illness was of the same nature." Of these 29 cases, 16 proved fatal. With the exception of only two cases, which occurred in a house immediately adjoining to that where the sick sailor died on the day of the *Hecla's* arrival, all the attacks took place among persons who either lived close to the wharf where the vessel lay, or who were at work during the day there; many of them had also been on board of her.

In the large majority of the fatal cases, none of the other inmates of the dwellings where the deaths occurred were attacked with the disease. Dr. Buchanan, in reference to this point, remarks:—"There were twelve centres from whence the disease had the opportunity of spreading, and many of these localities were perfectly adapted for the spread of contagious diseases; yet in no single instance out of all these did any person, whose business did not lead them to the infected neighbourhood of the docks, get yellow fever, or any disease at all simulating it." It only remains to be stated that the two pilots who had boarded the *Hecla*, the five seamen who helped to bring the ship into harbour, and most of the men employed in discharging the cargo, escaped any attack of the fever, although they had much more direct dealing with the ship than the majority of the persons who were attacked. Nor does it appear that the crew and the passengers who landed were afterwards attacked, or that they conveyed the disease to their homes. It had been urged, from the first, that the ship ought to be removed from the dock and taken to some part of the harbour at a distance from the shore, and from other shipping. This was not effected until the 28th of September, or nineteen days after her arrival; and she was then thoroughly washed out, in every part, with a solution of chloride of lime. She was not readmitted into the port for a fortnight. The weather, it should be added, was extremely hot at Swansea throughout September.

It is not to be imagined that the *Hecla* is the only vessel which has come to Swansea from the West Indies, having had deaths from yellow fever during the voyage, and landed persons, if not actually labouring under, at least convalescent from, that disease. Of recent years more especially, scarcely a season has passed without one or more instances of the

sort having occurred; and in 1865, prior to the arrival of the *Hecla*, two other vessels from Cuba had been admitted, in both of which one or two of the crew had died from the fever on the passage. Several of the medical men in the town remember cases convalescing from the disease having been brought into the port, but they never knew the disease to extend to a resident in the town.

I have only to add that, if we possessed such complete information on other similar occurrences as we do respecting the events at St. Nazaire and at Swansea—thanks to the able and full reports of M. Melier and Dr. Buchanan—much of the incertitude and inconsistency of opinion, still prevalent on many points in the history of yellow fever, would infallibly be soon dispelled.

In marked contrast with the quarantine measures adopted at Southampton in 1852-3, are the precautions which the Privy Council have deemed it necessary to enforce there during the last three months.

The *Atrato* arrived at Southampton on the 12th Nov., 1866, from St. Thomas, which she had left on the 29th Oct., and which was then suffering severely from yellow fever. There had been 13 deaths from the disease during the voyage; the latest had occurred three days before arrival. The latest attack of the fever had taken place on the 5th, or seven days before arrival. There were several convalescents from the fever on board; they were all so far advanced in their recovery as to be able to be brought on deck to be seen by the medical officer of quarantine from his boat alongside; for he did not now go on board, as he certainly did in 1852. On the present occasion, indeed, all the quarantine measures adopted were of extreme rigour; and even the paper, containing the replies given to the surgeon and captain of the *Atrato* to the queries put to them, was not returned to them for signature, but merely read over to them, after they had been sworn.

After the mails and specie had been landed, the vessel with all on board—a crew of 139 souls, and 61 passengers, of whom rather more than 40 were in the saloon—was ordered down to the Motherbank. While there, the sick with some of the crew were, on the evening of the following day, 13th, transferred in open boats to the *Menelaus* hulk, about three quarters of a mile distant; the weather was extremely cold with a gale of wind at the time, and considerable difficulty was experienced in consequence. Two gunboats were appointed to act as guardships of the *Atrato* and

Menelaus, to prevent all intercommunication between them, or with the shore or other ships. How long this precaution was continued, the public was not informed. The passengers, all of whom had remained healthy throughout the voyage, with the rest of the crew, remained in the *Atrato* for six days more, and were then removed to the *Parana* steamer, also stationed at the Motherbank, and continued on board of her until the 23rd, when they were released; and pratique being now granted to the *Atrato* also, every one returned with her to Southampton, after a rigorous quarantine of ten or eleven days from the period of her arrival.

The only casualties, which appear to have occurred after arrival, were these. One of the convalescents removed to the *Menelaus* on the evening of the 13th, had a relapse of fever, after the exposure in the open boat, on the following day, and died three days afterwards. Besides the death of an infant (but not from fever) of a lady recently confined, one of the *Atrato's* crew died on the same day as the first patient, the certified cause of death being intermittent fever. The three corpses were buried at sea, at the Motherbank.

Within a few days after the arrival of the *Atrato* at Southampton, one of the French mail steamers arrived at St. Nazaire from the West Indies, having had 14 cases and 7 deaths, among the crew, from yellow fever on the voyage. None of the passengers on board had been attacked. No fresh case occurred after arrival. The passengers were detained five days on board of a hulk, before pratique was granted. The period was therefore half of that required in the case of the *Atrato*.

The *Tyne* arrived on 24th Nov. Being one of the intercolonial steamers sent home for repairs, she had no passengers. Eight of the crew had been attacked with yellow fever during the voyage; and of these 4 had died, 2 had quite recovered, and 2 were convalescent. The latest death occurred on the 19th. She was sent to the Motherbank; and the healthy and the sick of the crew were transferred to two different hulks. Pratique was granted on the 1st of Dec., or six days after arrival. No casualty whatever had occurred.

The *Seine* arrived on 1st Dec. There had been several cases of yellow fever on board before she reached St. Thomas, and on the voyage thence to Southampton, 8 fresh cases and 2 deaths had occurred; the last death was on the 18th November. No fresh attack had taken place since the 17th. All the passengers on board (the number is not stated), had remained well. After the mails and specie had

been landed, having been previously fumigated, the vessel was sent to the Motherbank. The passengers and crew were detained for only 2 days; the ship herself was not released for 5 days more. No casualty had occurred.

The *Tasmanian* arrived next, on the 14th Dec. Yellow fever had occurred on board before she reached St. Thomas from the westward, and she had lost four of her crew in consequence. During the voyage thence to Southampton, 17 other deaths had occurred; and on arrival, there were 7 on the sick list, four of whom were not expected to recover. The latest death was on the day before she reached England. With the exception of one passenger, a medical man (who may probably have taken the place of the surgeon of the ship, one of the victims on board), all the passengers remained free from the sickness. After the landing of the mails and specie, the *Tasmanian* proceeded to the Motherbank. The passengers, who had been transferred to the *Parana*, and had continued entirely well, were released on the 19th, or 5 days after arrival. The ship was detained in quarantine, for thorough purification, for 9 days more. No casualty is mentioned (as far as I know), except the death of one of the sick crew on the 14th, the day of arrival.

The *Shannon* arrived on the 31st Dec.; all on board reported to be well. Only one case of yellow fever had occurred during the voyage, in the person of the cook; he sickened on the 17th, and died on the 23rd. Five cases of mild intermittent had occurred, but all the persons had quite recovered. The quarantine medical officer personally inspected the crew, and recommended that pratique should be granted at once. But the Privy Council directed that the passengers be transferred to the *Parana*, and detained on board for 24 hours before they were released. They were permitted to land on the 2nd January, so that their quarantine was scarcely two days. The ship herself was sent to the Motherbank to be fumigated.

The *La Plata* arrived on the 17th January, 1867. There had been on the voyage from St. Thomas 61 cases, and 23 deaths, from the fever; and four were on the sick list on arrival. None of the passengers, 34 in number, had been attacked. After the mails and specie had been landed, she proceeded to the Motherbank, and the healthy and the sick were transferred to different vessels. The passengers, all of whom had continued well, were detained in quarantine for 8 days, and the healthy portion of the crew for 9 days. The ship was detained in quarantine at the Motherbank for 11 days. No casualty appears to have occurred.

It would thus appear that of these six infected West India steamers which arrived at Southampton between 12th of Nov. last and the 17th Jan. in the present year, and on board of which 65 deaths from yellow fever had occurred at sea among an aggregate of probably between 900 and 1000 persons, crews and passengers, the only casualties in connection with this disease after their arrival off our shores were, as far as can be yet made out from what the public have hitherto heard, one death from relapse in the *Atrato*, and one or two deaths (perhaps more) of moribund sick in the *Tasmanian*, immediately after her arrival.

If we take into account the French mail steamer at St. Nazaire, 7 more deaths must be added as having occurred in the voyage from the West Indies to Europe; but there is no addition in her case to the casualties after arrival.

The exemption of the passengers on board the seven vessels, with the solitary exception of the medical gentleman in the *Tasmanian*, and who, probably, had attended the sick on board, is a striking fact; it will, of course, arrest attention. The same thing occurred, it will be remembered, in 1852-53; so that we have thus the highly significant fact of an all but complete immunity of at least, I presume, 600 persons on board of 11 vessels, in which yellow fever had to a greater or less extent existed.

We require, it is obvious, to have far more ample and accurate information respecting numerous details in respect of each ship, and of those on board of each ship, than we yet possess, to be able to appreciate, as is necessary in such an inquiry, the different influences, pernicious or salutary, which contributed to the seizure of some persons and the escape of others within so limited an inclosure as that of a ship. Among other points, it would be interesting to know whether any of the cases were treated under canvas on the upper deck; and if so, how many were so treated, with the results of these cases compared with the results of cases treated between decks. Suffice it, for the present, to say that the varied facts which have now been brought together, seem to fully warrant and to confirm the declarations of the late Sir William Pym, who will not be suspected of having underrated the virulence of the disease and the risk of its propagation, that "in an open airy situation, persons may approach a patient, perhaps even to contact, with very little or no danger," and that "the contagion is totally destroyed by cold, or even by a free circulation of cool, air." Like all unknown and unexperienced evils, yellow fever is apt to be regarded by those who are ignorant of its nature with exag-

gerated alarm; and this alarm is, of course, apt to give occasion for the adoption of needlessly severe quarantine precautions in the way of prophylaxis.

"For almost all practical purposes of prevention and arrest," it seems to me, "yellow fever may be regarded in the same light as the typhus fever of our own country. All know how much the virulence and diffusion of the latter are under control by wise sanitary precautions; and nearly the same thing may be said of the former. Both are liable, in a confined impure atmosphere, to spread by contagion from the sick to the well; whereas the risk of such an accident is reduced almost to zero in the airy wards of a clean hospital. The inference from this fact clearly is that, by the simple expedient of securing an ample circulation of pure air at all times, but more especially from sundown to sunrise, to every one without exception on board a sickly ship, the virulence and extension of the disease would be greatly controlled."*

The same hygienic principles should, I think, direct the procedures to be adopted towards vessels infected with yellow fever as are deemed necessary in respect of houses infected with typhus fever; neither more nor less. That there is, at the present time, a great want of a properly-organised medical police in all our principal commercial ports has been abundantly shown, of recent years, by such occurrences as the arrival of the Egyptian fever-smitten frigate at Liverpool,† in 1861, of the vessel in the Thames in 1862, and again of the *Hecla* at Swansea, in 1865. In the interests of the shipping itself, as well as of the population on shore, something ought unquestionably to be done to remedy this great defect; and when to this it is added that the sanitary condition of many of our harbours and docks is notoriously very faulty, it is surely high time that the whole subject, now briefly noticed, should engage more of the attention of the profession, as well as of the Government, than it hitherto has done.

* The Health of the Navy Considered, etc., by Gavin Milroy, M.D., London, 1861.

† Transactions of the Epidemiological Society, vol. i, p. 246.

P.S.—In October, November, and December of 1867. cases of yellow fever again occurred in three of the West India steamers, on the voyage from St. Thomas to Southampton, and the same rigorous and prolonged system of quarantine detention at the Motherbank was imposed upon each of them. No fresh sickness of any sort appears to have taken place among either the passengers or crews after their arrival; but from the want of any official information in respect alike of these vessels and of those mentioned in the preceding pages, the public is left in the dark as to the exact details. This *desideratum* should manifestly be supplied by the Government; it is only strange that it should have been so long withheld.

16 December, 1868. G. M.

ON SMALL-POX AND VACCINATION IN THE ISLAND OF ANTIGUA.

By THOMAS NICHOLSON, M.D.,

[*Read March 4th*, 1867.]

The prevalence of small-pox at the present day in the parish of Marylebone is calculated to raise grave doubts in the minds of the public of the value of the discovery of the philanthropic Jenner; or to impress them with the conviction that the Act for Compulsory Vaccination is shamefully neglected. To investigate the cause of this epidemic is no doubt one of the special objects of the Epidemiological Society, and it has occurred to me that the history of small-pox and vaccination in an island containing 36,000 inhabitants during the last fifty years may be deemed not unworthy of your attention. The limited field of observation has some advantages in such an investigation, inasmuch as it is more easy to trace effects to their causes than in a larger area with a more unsettled population.

In the Island of Antigua the practice of vaccination was early adopted, and the proprietary body readily consented to pay two dollars for the successful vaccination of each of their slaves. The black mother offered no objection to the performance of an operation on her infant which she saw performed on the infant of her master.

In the year 1834, when emancipation took place, most of the proprietors entered into an agreement with their medical practitioners for the attendance on their contract labourers and their children, including vaccination and surgical operations, for a fixed sum annually. For some years this arrangement was carried out most harmoniously, and the transition from slavery to freedom was scarcely perceptible. At length, one of our governors, a man of an amiable and philanthropic disposition, but obstinate in the maintenance of opinions not always accordant with correct principles of political economy, succeeded in interrupting the good feelings existing between the agricultural labourer and his employer. His Excellency was of opinion that the Negro

would never enjoy perfect freedom as long as he lived on his employer's estate, and, therefore, he urged upon him the propriety of obtaining a house of his own. The money which he had saved by a few years' of steady labour was, therefore, spent in the purchase of a few square yards of land of the cheapest description, on which he erected a small wooden house, seldom of more than two rooms, frequently only of one. Thus, in the course of time, about seventy villages sprung up, on ground generally marshy, and of no agricultural value, and built without any regard to sanitary principles. The relationship between master and servant was broken; the labourer and his children were no longer entitled to medical attendance; vaccination was neglected; and the mortality, especially of infants, became excessive. In the years 1837 and 1838 small-pox prevailed in several of the West India colonies, and it was the only disease for which, at that time, the laws of the island imposed quarantine. The regulations, however, were not very rigidly observed, for a ship arrived from the Clyde in December, 1837, in which the cook had died of small-pox, and some of the crew had suffered from the same disease during the voyage, yet it was at once admitted to pratique.

The same benevolent governor alluded to was opposed to the restrictions on trade which quarantine laws occasion, and he prevailed on the Legislature of Antigua to encourage protection by paying for vaccination rather than by paying for quarantine officers. The labourer was paid from the Colonial Treasury an equivalent for the loss of two days' labour, and the vaccinator was paid from the same source a fee of one dollar for each successful vaccination.

This liberal arrangement existed for one year only, for it was found that the draft on the Treasury was more than it could well afford to pay. However, after this, vaccination was still within the reach of the poorest individual, for it was offered gratuitously at the Colonial Hospital one day in every week. But, payment being stopped, the labourers seldom availed themselves of the offer, or, if they did, they seldom returned on the eighth day for inspection, therefore it was difficult to keep up a supply of lymph, and it was impossible to say that the operation had been successful. Still the colony had not been visited by small-pox for more than fifty years till the month of October, 1862. At that time the Board of Health had weekly meetings to guard against the introduction of cholera, which existed in some of the neighbouring colonies. Inspectors of nuisances had been

appointed, whose duty it was to visit every house in their respective districts, and report the occurrence of any case of small-pox, diarrhœa, cholera, yellow fever, or any disease of an infectious or contagious character, which they had discovered in the course of their visits. One of these inspectors reported the existence of some cases of chicken-pox in a village lying on the south side of the harbour of St. John. Varicella, so far as I have seen it in the West Indies, is a very mild vesicular disease, the vesicles always distinct, appearing in successive crops, and never becoming purulent, and, I confess, as a member of the board, I was guilty of overlooking the report of the inspector. The cases ought to have been inspected by the medical officer of the board, to ascertain the correctness of the diagnosis. It was not till a fortnight afterwards, when I was called to visit a child on an estate called Millar's, about six miles distant, whom I found labouring under confluent small-pox, of which it died the following day, that I became aware of the existence of small-pox in the island. The Board of Health was summoned immediately, and a report made to the Governor, who called a meeting of the Legislature, and an Act was passed to prevent the spread of the disease. Its introduction was easily traced to the arrival of a passenger in a small trading vessel from an infected port, who had evaded the quarantine law, and landed in the village referred to during the latent period of the infection. The mother of the child in whom I first discovered the disease had paid a visit to a friend in the said village, taking her infant with her. On her return to Millar's, fourteen persons of various ages, near neighbours of this child, were infected, some of whom had the disease in the most virulent form, but all recovered. They were kept in a detached house, about a quarter of a mile distant from the village of the estate, a yellow flag being hoisted on it to warn the public of the existence of the disease, and they were liberally supplied with food and attendance at the expense of the proprietor.

An Act for enforcing the practice of vaccination had been passed in July, 1862, and now endeavours were made to enforce the provisions of the Act more rigidly; nevertheless, the disease spread rapidly over the island.

When the malady occurred on an estate on which contract labourers resided, the same measures were adopted as on Millar's estate, and the mortality was not great; but when it occurred in the free villages, although they were supplied with food, medicine, and nursing at the public expense, yet the

mortality was great. For the accommodation of the poor resident in the City of St. John's and its neighbourhood, a small-pox hospital was erected on Rat Island, which was in constant occupation for several months. The expense incurred by this visitation was a heavy burden to the colony. I am not aware that any case occurred among the white population, or that any one of the coloured race was attacked, who had been properly vaccinated.

One interesting case occurred in which the wife was vaccinated as soon as the small-pox appeared on her husband. She took the variola, however, in a mild form, and both diseases ran their course together.

About the same time the legislature of Antigua passed an Act for providing medical attendance for the infant children of the labouring population, and for the poor and destitute, and to render such medical attendance accessible to the labouring population at large. The preamble is to this effect:—"Whereas an undue mortality, ascribed in a great measure to the want of proper medical attendance, prevails among the infant children, more especially of the labouring population, and it is expedient, with a view of arresting so great an evil, to make provision for medical attendance upon such children, and for the poor and destitute, and to render the same accessible, upon reasonable and practicable terms, to the labouring population at large." The island is divided into six districts, each, with the exception of St. John's (in which there is a public hospital and poor-law union), having a population of about 4,000. To each district a duly-qualified medical practitioner is appointed, with a salary of £150 per annum. His duties are to afford gratuitous medical and surgical aid to all children of labourers under ten years of age, and to all labourers over sixty; to cause all such children to be vaccinated; also, to afford, on the order of a justice of the peace or minister of religion, gratuitous medical and surgical aid, and the necessary medicines, to all destitute and sick persons within his district; to act as health officer for his district, and to make quarterly reports to the Governor. The 4th clause authorises the Governor in Council to frame a tariff or docket of fees, binding on the said medical officer, for services rendered to labourers and other patients in his district. Another clause defines the meaning of the term labourers.

This Act would be greatly improved if the system of village hospitals were added to it. At present it is not in favour either with the public or the medical men. The

farmers say they do not see the happy results they expected from it—the rate of mortality is not diminished. The medical officers complain that the Act pauperises the whole of the labouring population, for it is an easy matter to obtain an order for his services from a justice of the peace or a minister of religion; and that the tariff for those who are able to pay is quite inadequate to the services required of them. The Act will expire shortly, and is not likely to be renewed in its present form.

Before I conclude, I desire to record my testimony to the great success of the method of preserving vaccine lymph recommended by Dr. Husband, of Edinburgh. In the early years of my practice I was supplied with lymph by the National Vaccine Establishment, on dry ivory points, between squares of glass, and in tubes hermetically sealed, with a bulb at the other end. All of these frequently failed, and a vesicle was difficult to establish a true vaccine vesicle. When once it was established, it was easy to preserve the lymph for a few days between pieces of glass covered with tinfoil, but I always preferred vaccinating directly from the vesicle on the eighth day.

ON THE EARLY SEATS OF CHOLERA IN INDIA AND IN THE EAST,

WITH REFERENCE TO THE PAST AND THE PRESENT.

By JOHN MACPHERSON, M.D.,

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[*Read April 1st*, 1867.]

I. Early history of cholera in the East.
II. Names of disease.
III. Inferences. 1. Always of various kinds. 2. Long endemic in India. 3. Also epidemic. 4. Causes assigned. 5. Carried by ships.
IV. Its present diffusion in the East; inferences. 1. Tendency of disease to get fixed in adjoining countries. 2. Ships, caravans, pilgrims. 3. Deserts. 4. Imperfect information respecting the past.
V. General results. 1. Importance of Gangetic valley overrated. 2. Quarantine of Red Sea alone quite insufficient. 3. Habits of natives supposed to have an influence on the disease.
VI. Historical summary.
VII. Appendix. Historical table, and table of names.

THE disease called cholera morbus, or the choleric passion, has been fully described by most of the Greek and Arab authors. In more recent periods we have accounts of its violent forms, given by Sydenham, and Willis, and Morton, in England, not to mention Zacutus, Riverius, and many other continental authorities. The most comprehensive view of the whole subject was taken by the late Dr. W. F. Chambers, in his lectures in the *Medical Gazette*, 1849. Besides giving a full account of what Greek and Arab authors had said on the subject, he entered in some detail on the disease, as described by Sydenham and others. On this part of the subject Dr. Greenhow has also enlarged in the *British and Foreign Quarterly*.

For accounts of the disease within historical times in India we are indebted mainly to the Madras and Bengal reports of their Medical boards. The information supplied by them has been digested well by Dr. Chambers, by a writer in the *Times* in 1849, by the late Dr. Graves, and has also been given in abstract in Martin's work on *Tropical Diseases*.

But these accounts scarcely go back more than eighty or ninety years. The object of this paper is to give a sketch of the history of the disease within the period of European connection with India; to trace it from the very first appearance of the Portuguese in the East, down to the great renewed outbreak of the disease in 1817. I have to acknowledge my obligations to Hirsch's *Medical Geography* for many valuable references, and to Mr. Gaskoyn for some translations from the Portuguese previous to the date of D'Orta's work; and I have made free use of an article contributed by me to the *Quarterly Review*, January, 1867, containing some new facts, to which I have since been able to add.

I shall not enter in any detail into the history of cholera before the European period. There are full accounts in Sanscrit writings of a disease of various degrees of intensity, one of which was no doubt the true Indian cholera. These have been published repeatedly, and I need not go over the ground again. There is a certain want of precision in the descriptions given by Sanscrit, as by Greek and Arab writers, but the general truth of their accounts is unmistakable. Of late an inscription, said to have been discovered in front of a temple at Vizianuggur, describing the symptoms of the disease, has been resuscitated. But such things are often fabricated. Certain Sanscrit writings have been published in Madras, purporting to show that vaccination is an ancient Hindoo practice, but the scholars to whom I referred them, all doubt their genuineness. In the present instance it appears that the word *murree* is the one used on the inscription; now, *murree* means plague, or pestilence in general. Cholera has been called the sudden *murree*, and, doubtless, occasionally the great *murree*, but the latter phrase has been most frequently applied to an entirely different disease, a form of the Levantine plague. However, if the inscription be not authentic, we know from other sources that the disease prevailed in India before the European period.

I. The Portuguese first reached the coast of India in 1497, and they began to form settlements about 1502. Goa was founded in 1510. Their first proceedings in India soon involved them in war with the Samorin, the ruler at Calicut. and in 1503, during a campaign against him, there is a distinct notice of cholera, as well as of small-pox, having proved fatal to Europeans. In 1543 there was an epidemic of cholera of frightful intensity at Goa: a graphic account has

been left of the distress and consternation which it occasioned. The Governor found it necessary to put a stop to the tolling of the bells on each decease, as their melancholy notes added much to the prevailing alarm. I think it may be presumed that this is the first great epidemic from which Europeans have suffered in India, as it caused such universal consternation. However, such afflictions are soon forgotten; and D'Orta, writing at Goa a few years afterwards, does not mention the epidemic of 1543. In 1563 Garcia D'Orta, a physician, who had been at least a quarter of a century resident at Goa, published there the earliest work on Indian medicine, *Colloquios dos Simples*. Notwithstanding its numberless typographical errata, the work is most creditable to the Portuguese. D'Orta, it may be observed, does not hint at the disease being a new one. His account of cholera occurs in the colloquy on Costas. As it is the first full account of the disease given by a European, I have attempted a condensed translation.

"Let us approach the choleric passion, which the Indians call *morxi*, or disease from eating too much, and which we corrupt into *mordeshi*. The Arabs call it *hachaiza*, which has been corruptly read by Rhazes as *saida*.

"It is a malady which here kills very quickly, and from which few recover. It is more acute than in our lands, for it commonly kills in twenty-four hours. I have seen cases in which it did not last more than ten; persons in whom it lasted four days; and, as there is no rule without an exception, I have seen a man of strong constitution that lived for twenty days, went on vomiting bile incessantly, and after all died. I knew an excellent gentleman who suffered thirty hours from this complaint, and said that he had neither vomiting, nor purging, nor cramp, but was entirely prostrated by inability to breathe freely. The natives call this kind, got by excessive venery, the dry or seco.

"Those who eat much, particularly of cucumbers or shellfish, and those who have too much converse with women, suffer most. The disease is most common in June and July. Symptoms: Pulse is very weak; in a short time there is a feeling of great cold, with cold sweat; the cold is great while the patient complains of great burning; the thirst is clamourous, the eyes are very sunk, there is inability to sleep, much vomiting and purging, until the powers are so exhausted that nothing more can be expelled. Cramps in the legs follow. The patient turns and twists from suffering. After the patient may have been vomiting and purging a

couple of hours, at last he brings up only water, with no bitter or acid taste.

"The malady is not one that can be neglected either by the physician or hospital servants. As to treatment, there is a poisonous humour and infection which ought to be expelled and evacuated. The native treatment was to give a decoction of rice, with pepper and cummin, but, above all, to apply the actual cautery to the feet and ankles, and to tie ligatures round the limbs. He himself gave no water except a little in which gold had been extinguished. He used a variety of astringent vegetable medicines. Thought there was virtue in three grains of bezoar, a remedy with which he cured the Bishop of Malacca. He rubbed the whole body with hot cloths and warm oils, and, when the vomiting ceased, gave a little chicken soup."

Christopher A. Costa, also of Goa, writes in 1580 attributing cholera to the use of the jack fruit: as translated by Clusius, "qui frequentius Jaca vescuntur facile in pestilentem illum et pessimum morbum *mordshi* appellatum incidunt."

Linschott, a Dutchman, who spent many years at Goa, writes in 1789, according to the French translators, "Les maladies que ces changements de temps apportent aux habitants de Goa sont divers, entre lesquelles a la vogue celle qu'ils appellent *mordeshin*, qui survient en un instant et à l'improviste, avec soulevement de l'estomac et vomissement continuel jusques à tomber dans defaillance; cette maladie est commune et mortifere à plusieurs." After talking of the two next most fatal diseases, dysentery and fever, he adds of their fatality,—"Ces maladies font mourir annuellement grand nombre des Portugais." We thus have six distinct accounts in the sixteenth century of the common prevalence of cholera at Goa and the regions about it; or taking this in its most limited sense, the coast of Malabar and Canara. As the word *mordshee* is a Mahratta one, and as the Mahrattas are not the original inhabitants of Goa, their application of the name *mordshi* to the disease at Goa seems to show that the disease was previously known to them.

But in those days the Portuguese were in close connection with Muscat and Ormuz, and with Java and the Eastern seas. Cholera at present is readily conveyed by ships, and we should, therefore, expect to find traces of the disease elsewhere. Accordingly, within forty years of our last notice we hear of the disease both in Java and in Arabia. It seems to me probable that Zacutus' statement of the prevalence of cholera

in Arabia was published in Europe before Bontius' account of it in Java, if not written earlier. However this may be, in 1629 Bontius wrote a full account of the disease, which would have been unmistakable even if he had not identified it by mentioning more than once that the Malays called it *mordeshi*. Now, we know that the Dutch had been preceded by the Portuguese in Java—that the word *mordeshi* is not a Malay word, it must therefore have been imported from India, and applied either to a disease brought by them, or to a disease they found already existing in the island. Bontius has been so often quoted that he need not be reproduced here; but I would merely remark that he talks of the disease as a common endemic one—mentions four or five native remedies for it, and observes that the disease is dreaded as much as the plague is in Holland; but Zacutus's notice of the prevalence of the disease in Arabia at this time is particularly interesting—we know, indeed, that the Portuguese troops in various expeditions from Ormuz, suffered from sudden sickness, but the nature of such attacks does not seem to have been specified.

Zacutus was a celebrated Jewish physician, banished along with his countrymen from Lisbon, by Philip IV. He retired to Amsterdam, where he published the great systematic works of the day. He thus had an opportunity of communicating with the Portuguese as well as with the Dutch, who were the two nations in those days in closest connexion with the East. Letters addressed to him from the tropics, and especially from Dr. Pereira, of Goa, show that he was in communication with physicians abroad. His statement in 1632 is very distinct and positive—"Ne ergo contemnas hunc affectum, qui etsi in Lusitania nostra et Amstelodamo paucos jugulet, in Oriente ubi vocatur patrio sermone *mordeshi*, plures quos corripit extemplo jugulat, et in Mauritania et Arabia est lethalis fere, in quem affectum incidunt Arabes frequenter qui continuo jusculum esitant frigidum &c. quod cuscus vocatur." Prax. Histor. lib. ultim. Observat III.

Now this extract shows that Zacutus was perfectly aware of the tropical disease being more violent than the ordinary European one; he wrote forty years before the London epidemics, which by the way Sydenham says, differed "toto cœlo" from ordinary cholera. He knew that the disease was called *Mordshi* in the East, but he expressly adds that it was also common in Arabia—and goes on to specify the mess they eat, to which he attributes the disease.

We have nothing to say to his statement of the disease prevailing in Mauritania; it does not concern us at present, although it is likely enough to be correct—as it was described in that country as Almaida, by Averrhoës, in 1200.

Zacutus makes no mistake, and evidently knows what he is writing about. It will probably never be possible to ascertain what was the source of the disease in Arabia at that time: however, there is nothing surprising in its prevalence, as Rhazes and Avicenna, some centuries, indeed, before, both describe a cholera of very considerable fatality in that part of the world. Avicenna, who lived at Bagdad, attributes its prevalence chiefly to southerly winds, to rain and to heat; and says it is most fatal summer and autumn, and Rhazes calls it *saida*, another form of *haiza*, the word now commonly used in India to designate cholera: and besides this, the Arabs, according to Dr. W. Ainslie, have a tradition that the disease was introduced into Arabia some centuries ago from India—that it travelled over Persia, Syria, and Egypt, and finally disappeared in the African desert.

Mandelsloe, a German who travelled in India in 1638, adds little to our accounts of cholera, but mentions it as a disease that kills without delay at Goa. In 1666, De Thevenot, a French traveller, had a slight attack of cholera when travelling from Boorhampore to Surat, and takes occasion to explain that there are attacks of four various degrees of intensity, all described as *mordshee*. He first mentions this disease, and then gives an account of dysentery. In 1674, we have the first notice of the disease by an Englishman. Dr. Fryer speaks of cholera morbus as occurring in extreme heat at Surat, called *mordesheen* by the Portuguese—a vomiting and purging, treated violently by cautery.

In 1676, Dellon, a French prisoner of the Inquisition at Goa (and it does not matter to us whether his adventures are fictitious or not), attributes to his having been well-fed his escape from the cruel malady, called by the Indians *mordeshin*, which was frequent and dangerous in these climates. Dr. Dellon, about the same time, says that *mordeshin* is common in the Indies, that its symptoms are not always the same, that it causes death in a few hours if neglected. He attributes it to heavy food taken at bed-time, and hints at consecutive fever.

Pursuing the history of the disease, we must now transfer ourselves from the western, to the eastern portion of the great peninsula, from the palm-fringed coast of Malabar with its

luxuriant vegetation and back waters, and its magnificent back-ground of mountains, to the flat, bare, surf-beaten shore of Coromandel, from the haunts of the Portuguese to the Carnatic, where the great struggle for ascendancy was taking place between the English and French.

But it would be a very important link in the history of the disease, first, as showing it in full force above the ghauts, and, secondly, as forming another connecting link between the western and eastern sides of the peninsula, if we had any positive evidence of cholera having broken out in Arungzebe's army in 1689. We have, indeed, accounts of a violent epidemic having spread through his camp in the neighbourhood of Beejapore, and raging with such violence that no one reckoned on his existence for a single hour. To this, according to Grant Duff, the historian, Kafee Khan, has given the usual name for cholera, although the symptoms he mentions rather resemble those of the plague. In favour of the disease having been cholera may be advanced the statement of Linschott, that plague is unknown in India, that Beejapore is only 120 miles from the coast where we have so long seen the disease prevailing, and that when a disease resembling plague appeared in India, they gave it the name of *maha-murree*—great pestilence; a name very seldom applied to cholera.

Our next notice is from a Frenchman, a Jesuit missionary in Southern India, Père Martin, who was familiar with the disease between Madura and Trichinopoly. In his letter dated about 1702, he mentions violent gastric disturbance, with convulsions, and records his cure of the case by the application of the actual cautery to the soles of the feet, followed by violent slippering. An invaluable cure, he says, which was much used along the coast, but little known inland, or at Aour, where this case occurred. D'Orta, and De Thevenot, and Fryer, had already given an account of this remedy, and Dr. Dellon said that, after proving the inefficiency of all other modes of treatment, he had at last adopted it himself.

The history of the disease at this time would rather seem to indicate its having gradually crept north from Madura.

In 1709, another of the brotherhood, Frère Papien, writing from Hooghly, mentions *mordshi* as one of the diseases of the country. This is the first European mention of cholera in Bengal, but as for the present it stands nearly isolated, we must go to the parts of India, where we have fuller notices of the disease.

Madras accounts first mention the disease in 1756 at

Arcot, about fifty miles inland from the presidency town. It seems to have been a district to which the disease adhered for a long time, for in it or in Vellore, or in the adjoining valley of Amburpet, there are frequent notices of the disease from 1769 to 1783 and 1787. There can be little doubt that the disease mentioned by Orme as prevailing epidemically in Southern India, and causing sudden deaths in 1757, was cholera.* Dr. Paisley, of Madras, writing in 1774, says, that in the first campaign made in that country it was horribly fatal to the blacks, and that fifty Europeans of the line were seized with it, that it was the same disease which they had seen at Trincomalee. He says it is often epidemic among the blacks, whom it destroys quickly, and he approves of moving a camp to get rid of the disease. There seems no reason to doubt that cholera reached the Isle of France in 1775. The accounts of this given to our officers at a later period were clear and convincing. Of the ravages of the malady on the Coromandel coast and near the French settlements, we have full accounts from Sonnerat for the period of about 1775 to 1780. This writer has got the credit, but certainly without cause, of having converted *mordeshin* into the similarly sounding *mort de chien.* He gives an account of the ravages of more than one epidemic, and says that upwards of sixty thousand people of the country between Cherigan, in the delta of the Cauvery, and Pondicherry, were carried off by it. In the edition of his *Diseases of Seamen*, in 1780, Lind talks of the *mordeshin* as being very frequent and fatal in the East Indies.

Our next notice of the subject carries us a long way up the coast, to a neighbourhood of which we have scarcely yet heard, to the delta of the Mahanuddy and to Juggernath in the first place, and to Calcutta and the Gangetic valley in the second. Paxman, indeed, in 1736, mentions incident-

* It would be very important to get accounts of cholera epidemics in other parts of India of an earlier date, but no positive ones, except those of the Goa outbreak in 1543, have yet been discovered. Dr. James Johnson, indeed, once made a statement before the Westminster Medical Society, that there had been an epidemic in Surat in 1760, which carried off 60,000 people, but it does not appear on what authority this statement rests. A good deal has been made, especially by French writers, of the fact of 30,000 natives and 800 Europeans having been reported to have died of the disease in Bengal in 1762; but Lind, the authority for this, expressly calls the disease a putrid and remitting fever, to be cured by bark. Lind and Bogue both describe the diseases of seamen in the river Hooghly from a period which may be said to extend from 1757 to 1770, but they make no mention of any disease like cholera, although the disease must have continued to exist as an endemic, and Lind was familiar with the fact of the existence of mordeshin in the south.

ally that the *mordeshin* and *mordshie* occur frequently in India. He says he was nine years in Bengal, and visited Coromandel. He mentions bad fevers in Bengal in August, but says nothing of *mordshi* or of epidemics of it there. In 1781, a detachment of Bengal troops passing down from Cuttack to Ganjam were suddenly seized with cholera on the 22nd of March, the disease being said to have been previously common among the people of the country. It was entirely new to the Bengal officers, who, in the first place, as has happened in many a plague, attributed it to poison. The seizure was as sudden and as violent as its epidemic visitations continue usually to be. Out of a body of about 5,000 men, it is said that 700 died. As the detachment moved on to the south the complaint gradually left it; but the pest was not confined to the country near Ganjam, it travelled up to Calcutta, where, according to Warren Hastings, although recognised as the ailment called *mordeshin*, it created intense alarm and horror, having caused 879 deaths in ten days; in the month of April it gradually abated, and pursued its course to the northward. Unfortunately its course was not traced at the time, but it can scarcely be doubted that it is the malignant distemper of which Mr. Lindsay, of Sylhet, in the north-east corner of Lower Bengal, writes in the month of September, 1781, that, "after having carried off a number of the inhabitants of Calcutta, it is now raging with the greatest fury at Sylhet. Many of the Zemindars and Naibs having fallen victims to it, the others have in a body deserted the town."* The rumour mentioned by the Bengal Medical Board in 1819, that cholera prevailed in Bundelcund about forty years before, may have been well founded if the disease at this time travelled up the Gangetic valley; and perhaps this would in some measure render its sudden outbreak higher up the Ganges at Hurdwar in the early part of 1783 less surprising. The disease when it left the Bengal detachment on its way south, was not exhausted, for it appeared in General Andre's beleaguering army in 1781. We have ample evidence of its presence at Madras in 1782, when it killed fifty Europeans of a regiment within three days of their arrival by sea, and in less than a month a thousand Europeans suffered from attacks of it, in which year it also prevailed in the fleet off Madras, and at Trincomalee; and when Konig, the botanist, who gives an

* Taylor, Topography of Dacca.

excellent account of his attack, was near falling a victim to it at Tranquebar.

Next year, or in 1783, the Madras Report describes the disease as epidemic along the whole coast. We hear from Hay of its being in the south, in the Travancore country; and it showed itself in the army of observation. In this year, too, Curtis talks of *mort de chien* as "this fatal, intractable Indian disease." Early in 1783, or about eighteen months after our last notice of cholera in Lower Bengal, we meet with a furious outbreak of it among the pilgrims assembled at Hurdwar, at the very head of the Gangetic valley. It is said to have carried off 20,000 men in a few days; but, with the apparent capriciousness which distinguishes it, spared a village only a few miles distant. We have no account of the cholera spreading on that occasion.

It appears to have abated somewhat of its violence after this, although we still have notices of its ravages. It is at Vellore and Arcot, which are close to each other, in 1787; in 1790 it again attacked a detachment of Bengal troops moving south near the Chilka lake, in Ganjam; and in 1797 a collector, in one of his reports, alludes to the sickness and mortality in a pergunnah of Backergunge, in the delta of the Ganges, and states, that "in one house, that of a grain-dealer, seventeen lives have been lost in eleven days: and I consider that from four to five hundred lives have been sacrificed to this plague, which has not yet been subdued."* This can only refer to cholera; and tallies in date with the Report mentioned by the Bengal Medical Board, that the disease prevailed epidemically in Lower Bengal in the end of the last century. Yet when it reappeared there, only twenty years after, it was at first taken for a hitherto unheard-of pestilence!

Here we may pause to examine the main features of the disease. We have it attacking soldiers on disembarkation, and sailors on board their ships; troops marching, and troops in camp; we have the holy fair at Hurdwar; we are in Cuttack, on the track of pilgrims to Juggernath; at Vellore, in the route of those going to Conjeveram; at Cherigan, of the frequenters of the southern shrines and Ramisheram. We have the disease freely pervading Ceylon, and, in all probability, carried to the Isle of France. Finally, we have it showing itself along the backwaters of Travancore, up the western coast to Goa, Surat, and the high

* Taylor, *suprâ*.

country above it; in the delta of the Cauvery; in Vellore, surrounded with its rice-fields; in the delta of the Mahanuddy; and in the delta of the Ganges, where it was not destined to take permanent root in its worst form yet awhile. While one is struck with the obvious fact that these accounts refer chiefly to the sea coast, and to districts not very far removed from it, we must also remember that Europeans had very few dealings in those days with the interior.

On looking back to the prevalence of cholera from 1756 to 1797—at least so far as we are enabled to do so by the light of our imperfect accounts—the period of the commencement and termination of the wide-spread of the disease in India during the latter half of the last century, we may assign to about the middle of this period, or from 1775 to 1785, and particularly to the years 1781-82-83, the maximum of intensity of its epidemic diffusion.

Our notices of cholera for the next nineteen or twenty years are scanty in the extreme; there was evidently a period of quiescence. Dr. J. Johnson saw it in the harbour of Trincomalee in 1804. Mr. Barnes, of Jessore, which lies about sixty miles from Calcutta, says that the Court at that place had on two occasions previously to 1817 been broken up, owing to the outbreaks of the disease, and that he remembered having seen cases which resembled cholera; but this is very vague and indefinite. Very recently Mr. C. Macnamara has exhumed a few notices from the records of the Bengal Medical Board, which show that in the years 1808, 9, 11, 12, 13 and 14, also in March, 1817, stray cases of the disease had occurred in various military stations, most in Chunar, near Benares, but some in Fort William. There are very clear accounts from three Madras medical officers of a slight outbreak of cholera in some native troops marching near Jaulnah in 1814; and the next evidence of an absolutely reliable character* that we have, shows cholera in 1817 already widely spread in the delta of the Ganges: in May and June in Kishnagur and Mymunsing; in July at Sonergong, in the Dacca district, and as high up the river as Patna.

Now, it is not common for any pestilence to break out suddenly in country districts where there has not been some unusual collection of people. We do not know of any great

* Corbyn's very extraordinary statement that he saw the disease on board the *Mangles* Indiaman on his way from England to the Cape in 1814, when there were 64 deaths among the Lascars, has never been accepted.

assemblage having taken place in Bengal that year. There is not the faintest reason to suppose that there was cholera in 1817 at Juggernath. Even if there were, the districts where it first showed itself are out of the line of pilgrims, and the disease burst forth before the date of the great religious gathering of the year. According to all analogy—just as we had stray cases of cholera in England in 1865, but our epidemic did not come till 1866—we should expect to be able to discover traces of the disease in Bengal before it became so general. Such traces have been found by Mr. Macnamara, but this does not detract from the extreme interest of the communication of an anonymous writer to a Calcutta newspaper in 1831. He states that a band of bird and fruit sellers—in fact, a sort of gipsies, called Kooroorceas—were seen by him at a village called Saibgunge, in the district of Purneah, north of the Ganges, in 1816, and that in the months of April and May they suffered from a pestilence unknown before as an epidemic, which consequently had no name, but to which they gave the name of *oola*;* that it killed eight or ten of them a day, and that in consequence of it they moved their encampment. We are inclined to attach every credit to this statement, which is put forth quite simply, and unconnected with any theoretical views. The disease is represented as breaking out at the very season which we know to be the commonest one for cholera in that district, and among a class of people, wanderers, among whom we should expect it, and just the people to diffuse it. There is a great probability that they did spread the germs; for it is in districts not far removed from Purneah, that we find the disease first showing itself next year.† How these people got cholera, we cannot pretend to say. We do not know whence they had come, or how long they had been in Purneah; but, granted that they spread the germs of cholera in 1816, the outbreak in 1817 is less surprising. We do see something like a commencement to it, and without the help of these gipsies we have no way of accounting for the disease being widely spread in the earlier half of the year in Northern Bengal. There is no evidence of a spread of cholera from any one central focus in 1817.

As the effect of pilgrimages has of late been brought for-

* Olá-uthá, now the common Bengalic name for cholera, is defined in Forster's vocabulary, 1802," Flux, attended with vomiting."

† There are many villages called Saibgunge in Purneah. It may be said in a rough way that Patna is about 150 miles to the west, Mymunsing the same to the south-east, and Kishnaghur the same to the south of Purneah.

ward so prominently, as pilgrims undoubtedly frequently carry back cholera with them from Juggernath—it may be well to point out that Calcutta and Lower Bengal generally lie out of the main track of pilgrims. Pilgrims passing by Gya, Deoghur, Bancorah, and Midnapore to Juggernath keep quite clear of Calcutta; and in the year 1818, in particular, the Bengal Report says, "Of late the disease has appeared much in the south-west parts of Bengal, visiting severely Midnapore and Cuttack, which it almost spared in the first year of its existence." Lower Bengal, indeed, has scarcely any places of pilgrimage; and Kali Ghat in Calcutta, and Saugor Island at the mouth of the Hooghly, attract a comparatively small number of votaries. So far, then, as we are able to throw any light on the question of how the cholera outbreak of 1817 originated; it is very plain that the disease did not come up, as in 1781, from Cuttack; it is also clear that it first appeared in the upper part of the delta of the Ganges, nay, probably north of it; it possibly may have come down the Gangetic valley to Bengal. But our information as to the presence of cholera in Upper India at that time is too scanty to help us in the solution of the question. Certain it is that cholera in its worst epidemic form was fairly introduced into Bengal in 1781, but, for some reason unknown to us, did not fructify then, as it has done since 1817; and it is probable that if introduced at all in that year, it was entirely from a different quarter from that from which the visitation of 1781 had come. In 1817 both Europeans and natives had almost forgotten the former epidemic. So soon is the memory of such things lost.

Yet in a certain sense the disease cannot be said to have been a new one in 1817 in Bengal, for we have seen that cases of it had been occurring occasionally from the beginning of the century. Dr. Macrae, writing from Chittagong, informed the Medical Board that he had had occasion to observe the disease, as it prevailed in his district more or less every hot season. Indeed, the Board itself in its early communications to Government, assured it that the malady was only an aggravated form of the usual epidemic of the season, though they did not in the first instance apply the name of cholera to it.

No question in medicine is more interesting than that of an endemic disease taking on the character of an epidemic, and of the behaviour of an endemic, when its own epidemic form reaches it.

Returning to our earlier ground in Western India, Dr.

Clarke, in 1772, talks of cholera as a very frequent disease at Bombay. It appears to have been common in Ceylon from 1774 to 1782. In 1776, Fra Bartolomew found the disease on the Malabar coast; and he describes a bad epidemic of it in 1782. In this same year, according to Dr. Clarke, European troops died at Bombay of it, and of coup de soleil. Dr. Clarke, in his edition of 1792, in the new chapter, says, the disease is common in Malabar and Canara. From this time to the first outbreak of the disease in the Bombay presidency, and in India generally, in 1817-18, we have scarcely any information about it in the west. It is astonishing that the Bombay Medical Board had so little to say about the former prevalence of the disease. They only contribute the rumour mentioned by Mr. Jukes, that the disease had prevailed in the Mahratta country about 1790, 1794. They are not even aware that cholera had prevailed in 1814 near Jaulnah, scarcely 200 miles distant.

Still there is sufficient evidence of cholera being endemic at this time. Sir J. Malcolm states that the bad form of it was always endemic in the jungles of Malwa—some distance, no doubt, from the western coast, but still in constant communication with it. Dr. W. Ainslie says that cholera morbus was very common on the Malabar coast. Dr. Macrae writes to the Bengal Medical Board that he had personal knowledge of the prevalence of the disease in that district since 1790. Mr. Craw, writing from Seroor, states that in 1818 common cholera had been prevalent during the rains at Caranja, but that when the epidemic disease arrived next season, the nervous symptoms were so marked, that he considered it resembled tetanus rather than cholera. Mr. Hay's statement from Travancore in the south, also is very interesting. He writes, Nov. 19, 1818, from Quilon: "The spasmodic cholera, I am happy to say, abates the last seven days, having only afforded thirty-six cases, and one casualty, but the Vythians report the death of almost all attacked. I trust to be able to make a noble stand against the epidemic when it arrives: what I have had to encounter recently I hold to be the endemic, if not of the Malabars, certainly of the Travancorians, which is perfectly familiar to us all. Twenty-five years before, it devastated the country, destroying thousands." No new disease, however, did reach him this season, and Mr. Scott, the author of the Madras Report says, that there could be no doubt that the endemic of Travancore was the epidemic of other parts. We have thus traced an endemic cholera prevailing at times with epidemic violence

in the western and southern portions of the Indian peninsula, from the earliest European times down to 1818—and it is not till that period that there is the slightest occasion to allude to Bengal, or the distant delta of the Ganges.

I shall pause here for a short space, and sum up the characteristics of the disease as it manifested itself before 1817—some of those bearing on its ætiology. If we inquire what was the general configuration of the districts in which cholera showed itself, it must be admitted that it has been very various.

It has been found along the back waters, and at the mouths of the tidal rivers, and generally along the sea coast of the west and south of India—a district remarkable for luxuriant vegetation and for periodical heavy rainfalls. It has also visited the deltas of the Mahanuddy and of the Ganges, the conditions of which are tolerably similar;* but then it has also visited the upper Gangetic valley, which is dried up for a great portion of the year, and where the rainfall is scanty, as compared with other parts of India. It has also prevailed with great epidemic violence for a series of years on the plains of the Carnatic, and along the eastern coast of India, which is in all respects the opposite of the west; and it has flourished on high table land, that of Malwa, 2,000 feet above the sea, being probably one of its oldest seats.

It has existed on alluvial soils, on laterite and trap and primary formations, under the most various conditions of sub-soil and of drinking water.

It has always had a tendency to appear at particular seasons of the year.

It has shown a fondness for towns and places of pilgrimage; for soldiers and for camps; for shipping, having been readily carried by ships; and on land showing a power of travelling over long distances in a very short time.

The bearing of these facts on some of the conclusions of the Constantinople Conference will be apparent. That body declared that "Cholera seems to be an original product of the valley of the Ganges. The cholera of the invading character, which we have in our days, is necessarily the result of new conditions, which have been produced in India about the year 1817. Cholera's being only of late years in a state of permanence, must be due to some new and special condition of these localities; some special peculiarities as to houses or food,

* It may be mentioned that old writers generally describe the climate both of Malabar and of Bengala as healthy.

or recently-acquired habits—for instance, the burying of the dead." So little did they know of the history of the disease!

II. We have found the usual native name in those days for cholera, *mordeshi*, a useful guide in our notices. We may inquire into the history of that word, and also of *haiza*.

Rhazes appears, about the year 900, to have used the latter word under the form of *saida*—h and s are letters easily convertible. We again find the word used for cholera by Averhoës about 1200. He terms it *almaida—i.e., alam i Haidsa*, or disease of *haidsa*. In process of time *haiza* has become the common word for cholera all over the East. Its original meaning seems to be vomiting and purging. It need scarcely be said that the word is Arabic.

The other name, now almost forgotten, though it was the one universally applied by Europeans, *mordeshi*, is *morshee*, the word found by the Portuguese in use at Goa, where the Mahratta dialect prevailed. After a good deal of inquiry among Eastern scholars—Col. Chambers, Dr. Hall, and Dr. Rost—I do not think that either of two derivations from the Sanscrit, or Dr. Craigie's derivation from the Persian, making out the word to be "death of the bowels," are at all satisfactory. For the present, the derivation from the word *mödna*, to tear or twist, is about the best. The word still means in Mahratta cholera, but in Guzeratee at the present time it only means pain in the stomach. Dellon, in 1682, writes of the *mordeshin;* Martin, in 1702, says the French had corrupted it into *mort de chien*. This, therefore, is the probable date of the transformation of the name.

In India there seem always to have been various forms of cholera, but the more violent one came to be known as *mordechi* by Mahrattas, as *haiza* by Arabs, as *cholera morbus* by Europeans; in each case the term meaning the disease of cholera par excellence. Just as the Europeans applied the old name of *cholera morbus* and the Arabs the old name of *haiza* to the malignant disease in India, so does no epidemic of it in India appear ever to have originated a new name. On the outbreak of 1817 the Bengalees did indeed coin a new goddess, but they called her by the old name, *Oolabeebee*, from *oola* flux. I believe all the native names of cholera (except one synonymous with epilepsy, and said, rather doubtfully, to have been employed at Bombay) mean vomiting and purging. This survey warrants us, I think, in drawing two conclusions; first, that cholera morbus, or malignant cholera, was considered to differ only in degree, not in kind, from other forms of the disease; second, that vomiting and

purging were always recognised as the two prominent symptoms, just as *brechruhr* is the common name for it in Germany. Some of the synonyms of cholera will be found in a note at the end.

III. I think that several conclusions may be fairly drawn from the foregoing history of the disease and of its names, and the first of them is—

1. That cholera of various degrees of intensity has always prevailed in India, as it does at the present day. If we refer to old Hindoo medicine, we find that under the head of *Ajerna* were described four kinds of cholera, the worst form of which was the *vidhuna visuchi*. [Dr. Hall, of the India Office, assures me that this, like other phrases for cholera, merely means in Sanscrit a disturbance of the stomach and intestines.] This division seems to have passed over to the Portuguese, for, curiously enough, we have De Thevenot saying there were four degrees of *mordeshi*. Different writers make different distinctions among choleraic attacks, but the following are the common forms met with in India, and I may remark that I have had occasion to treat the disease in each of twenty-four consecutive years. You have ordinary bilious attacks of vomiting and purging. You have much more violent choleraic attacks, sometimes fatal ones, often connected with some article of food that has acted as a poison. The earliest Portuguese writers mention such attacks after eating prawns, and I have seen various such cases myself. You have true cholera endemic in districts, occurring sporadically, and generally showing a tendency at particular seasons to become epidemic; and you have the same disease raging with epidemic violence. The symptoms of all these forms of disease run into each other. In their commencement there is no absolute line of demarcation between them, any more than between European and Indian cholera, They only vary in the intensity of the symptoms and the amount of prevalence. Of course no one can mistake a well marked case of the bad form when it is fully developed, but not every one can say where the mild form ends and the bad commences; nor at the outset of a case can you always predicate absolutely, although you generally can, which it is to turn out. These appear to me to be simple facts, whatever conclusions may be drawn from them. I speak of symptoms only. I do not enter into the question of the specific character of malignant cholera.

But a division of cholera of the violent form into two kinds is brought prominently before us by the history of the

past: cases of ordinary Indian cholera in which vomiting and purging were the prominent features, and those in which affections of the nervous system, indicated by spasms and convulsions, were the more marked symptoms.

The latter form was described by Hippocrates as the cholera *sicca*, and led the Greek and Arab physicians to recommend the application of ligatures to the limbs in cholera; a practice derived from their ordinary treatment of epileptic convulsions. So prominent were those features even in the earliest times, that Sanscrit writers disputed whether cholera should be classed among diseases of the nervous or of the digestive system. Bontius in Java, and Girdlestone in Madras confounded cases of spasmodic cholera with the idiopathic tetanus of the country. When the violent epidemic of 1818 broke out in Bombay, we have seen Mr. Craw stating that he thought the disease had more of tetanus than of cholera in it; and at that time one of the names applied by natives to cholera seems to have been *mirgee*, the common phrase for epilepsy. However, as Dr. Clarke sensibly remarked, the identity of the two diseases was best shown by all finding the treatment for cholera to be the best for the tetanic attacks. Attacks in which there appears to be a sudden impression made on the nervous system, seem to occur in epidemics of all diseases, as, for instance, in the Levantine plague, and in the suddenness with which boats' crews were attacked with fever in Bengal in 1762, according to Lind. Cases of cholera, in which the patient is described as struck down at once, always have occurred, and no doubt continue to do so. I have not seen them myself, and the more such cases are examined, the fewer I believe will there be found to be, in which the usual changes in the intestinal canal have not commenced, if they have not been fully developed. I believe the far greatest portion of these cases are merely ones that have been imperfectly observed. Still the occurrence of such cases cannot be doubted.

2. The disease known as Indian cholera has existed as an endemic in the East as long back as we have any certain European accounts. Our early histories do not, indeed, always give full and systematic descriptions. Particular symptoms, such as suppression of urine, rice water evacuations, secondary fever, are often not mentioned at all. But the disease was one which at once arrested the attention of strangers. It is mentioned by Linschott as the first and chief Indian disease, before dysentery and fever. This could not have

been the case if it was only the ordinary European cholera. It is spoken of as a cruel pest; as coming on quite suddenly, and with no warning; as being dreaded as much as the plague in Europe; as killing most of those it attacked; as often killing in a few hours; as prevailing at particular seasons; and as being very fatal both to Europeans and to natives. I think that no more detailed proof (even if we had not examined the history of the word *mordshi*) need be adduced to show the violence of the disease, and of its endemicity we have had ample evidence; that it was always endemic in the west and south of India; and that Europeans have suffered from it from the period of their earliest connection with India.

3. One great characteristic of Indian cholera has been called by the recent Constantinople Conference its invading character, or power of spreading, or of epidemic diffusion. It is, indeed, extremely difficult to draw any positive line between endemicity and epidemicity. Our early notices of the disease represent it as endemic, with a tendency to become epidemic; as not limited to India, but known in Java and Arabia. In India epidemics of all kinds have been mentioned in such vague terms (as *jurree murree*, sudden death) that it is difficult to identify them. It is, therefore, particularly fortunate that we have the Portuguese account of the epidemic at Goa in 1543. Sir T. Roe, about 1616, says the Emperor would not go to Agra on account of a plague there, but says nothing of its nature. Passing over the very probable, but not quite certain, account of cholera breaking out in Arungzebe's army in 1689 before Beejapore, coming to positive proof of its epidemic prevalence on the large scale at a later period, we have supplied ample proof of the disease having been widely epidemic on the Coromandel and Malabar coasts, in the middle and latter half of the eighteenth century, even without pursuing its course up the coast from Madras to Bengal. Any increased power of spreading in the west and south, if admitted, is no new feature of the disease. It is at most an old one intensified, just as the disease has not shown any single symptom since 1817, that had not been often observed before. It is, indeed, a matter of fact that about the year 1817 the disease, after a period of comparative slumber, awoke to a fresh period of prolonged activity, which continues to this day. I confess that after a good deal of study of the subject, I have not been able to arrive at any satisfactory reason for the fresh outbreak of that year.

At the time of the outbreak much importance was attached to food—to bad rice, and to decayed fish; but it was never shown that with respect to them there was any state of things in 1817 that had not often existed before in Lower Bengal. We know that bad food predisposes to many diseases, and that some kinds of it produce in Europe, as in India, attacks, and often fatal ones, most closely resembling cholera; but we have no reason to suppose that they generate more than local epidemics. The state of the weather previously to the outbreak was investigated, and it seems to have been ascertained that the seasons had been very irregular, and that there had been extensive inundations; but although it has been from the first very clear that cholera bears a distinct relation to season, and that its diffusion is aided or impeded by meteorological changes, but few have considered it probable that any such atmospheric irregularities as have been observed, could have produced the great outbreak of 1817.

Then it has been said, and undoubtedly with truth, that increased intercourse helps the spread of cholera, and the greater tendency to spread is the only fresh characteristic of the malady in 1817; and it has been attempted to connect increased communication in India with that particular period. We know of no good grounds for this, except the assembly of the large army of the Marquis of Hastings in that year. But Lower Bengal, and more especially the portion of it where cholera showed itself first in 1817, is entirely out of the course of the usual movements of Indian armies—we have already seen that it was out of the direct course of pilgrims; and as to increased communication and travelling in India, the improvement in them has been a gradual process; and it appears to be a mere arbitrary assumption to assign the period of 1817 in particular as the date of its commencement. The first steamboat did not reach India till 1826, so it is a mistake to suppose, as some have done, that steam helped to convey cholera to the Persian Gulf, which it reached in 1821.

It has been a favourite French notion to throw the onus of the production of cholera in India on English domination, and to attribute it to neglect on the part of the English Government of the great canals and works of the Mahommedan emperors. I need not inquire where those great works were situated, or at what period they fell into decay. It seems sufficient to remark, that cholera is first known to us in districts in which there never were such works, and that its great centre at present is in a part of India where none such ever existed.

It seems on the whole very improbable, especially with reference to the history of other diseases, that we shall ever be able to ascertain why the very invading character of cholera, as the Constantinople Conference terms it, should date from 1817.

4. In those early days the causes assigned were almost always indigested food, and sometimes venereal excess—and a good deal was attributed to the season, and to the air.

Avicenna, at Bagdad, said, "Auster et multitudines pluviarum et regiones meridionales solvunt ventrem." Dorta talks of cholera prevailing in the regions about Goa in June and July. Linshott says it is brought round by the seasons. Bontius, in Java, talks of the influence of heat. Bartolomeo talks of the prevalence of the disease on the Malabar coast in October, November, December, saying that its season was different elsewhere. This character the disease has maintained. Though it may break out, and there may even be partial epidemics at almost every season, each district in India has its season when cholera prevails most frequently.

I have not come across any trace of its being thought contagious or infectious in early notices of the disease.

5. In those days cholera was propagated across the seas, and there can be little question but that this was by ships. At first sight one might be inclined to say—we know that this is the case in these modern days of quickened intercourse—but we should not think it probable in these earlier times; but the more one looks into the question, the greater does the intercourse seem to have been in those days. Every ship, whether Portuguese, Dutch, English, or French, seems in the earliest days to have touched at Zanzibar, at Aden, or Muscat, or at some port on that coast on its voyage from Europe to Surat and Goa. From the western coast of India the voyage was usually extended, especially by the Portuguese and Dutch, to the Moluccas. Then the Portuguese of India were in close intercourse with their settlement at Ormuz. Ships sailed from Surat, conveying merchandise from Agra and Bengal, brought to be shipped for the Persian Gulf, and go on by caravan to Aleppo, the overland route of those days. In the earliest times of the Portuguese the fleet of the Caliph of Egypt even issued from the Red Sea to attack them. European vessels often went up the Red Sea. I need hardly add that from the earliest times pilgrims sailed annually from Surat and other ports, bound for Arabia. The accounts, therefore, of the disease having prevailed both in Java and in Arabia by Bontius and by Zacutus, are only what we might have expected to have.

The introduction of cholera into the Mauritius and Bourbon in 1775 is easily explained, as we know that the malady was common in the English and French ships on the coast of Madras, and in Ceylon; and that the Mauritius settlements of the French were in constant communication with the Indian ones.

IV. We may next turn to another portion of the subject, and inquire how far these districts of India in which we have the earliest notices of cholera, or in which the great outbreak of 1817 first showed itself, continue to be seats of the disease. On this subject my information is not very complete. In the absence of complete statistics, general impressions must often be trusted to. Still, for the present, the case may be thus provisionally stated.

Since 1818, Bombay has been a favourite seat of cholera. It must have spread with the extension of that city. In the years 1862, 63, 64, 65, the deaths by it have averaged more than 3,000, and in one year they nearly reached 5,000. In the last two years the number of deaths has been considerably reduced. Taking the west coast as a whole, the disease does not seem to be very widely spread. It occurs frequently at Surat, and occasionally at Vingorla, Cannanore, and Cochin. It has, according to Inspector-General Pearse, been at least five times more common among our troops on the comparatively high tableland of the ceded provinces, than on the coast of Malabar and Canara. He thus expresses himself: "During a period of fifteen years at Cannanore, where the aggregate strength of the European force was 14,441, only fifteen deaths occurred from cholera, that of native troops 49,389, only sixty-one deaths. This immunity of the troops is not easily explained, for the native population generally suffer severely during epidemics. In the latter part of 1859 the disease prevailed throughout the provinces of Malabar and Canara, destroying large numbers of the population. In the jail at Calicut, a well-ventilated and commodious building, in which 400 prisoners were confined, cholera in a few weeks swept away more than one-fourth of its inmates. A detachment of European infantry, numbering one hundred men, escaped at the same time without losing a man." I observe that there was again in 1863 a fatal outbreak in the jail at Calicut, and only last year in 1865 there was a great epidemic in Malabar, which killed 14,000 people in one month.

The notice given by Thevenot of cholera occurring near Boorhampore is particularly interesting to us, taken in con-

nection with Sir J. Malcolm's belief that cholera was always endemic in the Malwa jungles. If that was really the case, the prevalence of cholera in Bundelcund about 1786, is easily explained, and even the outbreak of the disease in 1783, at the more distant Hurdwar, is less surprising. In 1841 Dr. Irving, in his topography of Ajmere, reported that the neighbouring districts of Kotah Boondee and Merwar are almost annually subject to its severest visitations. It was very prevalent in the south Mahratta country in 1841-42. The recent accounts of the disease breaking out in troops near Mhow, has again painfully directed attention to this particular part of India, which, by the way, has also been remarkable for its sweating sickness. On the whole this district, which offers a complete contrast to the strip of land along the western coast, and to the deltas of rivers, is one of the oldest and most persistent foci of cholera in India, and it is 1500 to 2000 feet above the sea.

But coming back a little nearer the coast of the Concan and Canara, if cholera is not at present very widely spread along it, yet almost all the principal towns above the ghats suffer from it nearly annually, and chiefly in the hot weather. It is enough to enumerate (not forgetting Baroda, which is low), Sattara, Sholapore, Seroor, Dharwar. I think we have thus conclusive evidence that if cholera, in these as in other parts of India, had become less frequent before the outbreak of 1817, yet the disease was only reintroduced into a soil in the west of India, where it had prevailed for centuries, and where it seems as ready to flourish now as in earlier periods.

The portion of India known as the Madras Presidency, and in a general way representing the eastern coast of the Peninsula, was a constant seat of the disease from the middle of the 18th century, and for a period of about thirty years its epidemic ravages in that part of India were frequent. At the present day it does not, however, appear to suffer as much as Bombay or Bengal. The town of Madras, indeed, often suffers from epidemics, and cholera appears periodically at different places along the coast; it also occasionally shows itself on the high plateau of Mysore. But another and less elevated plateau, known as the Ceded Districts, appears of late to have been its favourite seat, at all events it has attracted most observation. Madras regiments, both European and native, have a tendency to pick up cholera on the line of march in a proportion quite unusual in troops in other parts of India. This is usually explained by the immense number of women and children and camp followers

that commonly accompany them. Outbreaks of cholera in this part of India are often connected with religious festivals and gatherings.

In Lower Bengal cholera appears unfortunately to have struck its roots very deeply. It is never absent from Calcutta, and epidemics in the different districts of Lower Bengal are frequent. It is believed to be now endemic in many of the large cities of the north-west provinces, though probably not yet fixed in the Punjaub, and epidemics are not infrequent in the north-west. European soldiers appear of late to have suffered in much larger proportion than the native troops or native population. In Bengal as elsewhere pilgrimages bear their share in the spreading of the disease. And there are frequently epidemics at Gya, Allahabad, and Hurdwar. In most cases, I believe that the travellers bring the disease with them to such gatherings, and that the spread of the disease depends on the season and other influences bearing on the propagation of the disease, among which must be included the prompt application of hygienic measures.

The seasons of cholera have been tolerably well ascertained in Bengal. In the lower provinces the great season is the hot weather, there is almost always a large diffusion of the disease at this period, the shipping in Calcutta suffering especially. In the early part of the cold weather, especially in the eastern districts, the disease appears to have a new period of increase in the commencement of the cold season; as we pass to the north-west, the period for epidemics appears to be retarded, and it occurs after the setting in of the rains, not that it does not occasionally come in the hot weather. In the Punjaub it is still later in the year. But in the north-western provinces the disease is almost extinct in the cold weather, which is by no means the case in Lower Bengal. I may add that besides the two periods of increase just indicated, Dr. Bryden, who has studied the statistics of the subject with much care, is inclined to believe that there is a third intermediate period of disease, about the commencement of the rains.

The history of cholera in Java is curious. It is spoken of in 1629 as a prevalent endemic greatly dreaded. There can be no doubt of its virulence. A French writer refers to its having been epidemic there in 1689, but I have not been able to verify this. However this may be, it seems to have died away in its worst form, although there was always a slighter form called *mountan* by the Malays, which they

treated with strong coffee, until we hear of the disease reaching the island in 1819 from Bengal. It prevailed there again from 1826-30, and we lose sight of it again till we hear of it at Sumatra in 1853. In Malacca there were epidemics in 1819, 21, 26, and 40, and the disease again prevailed in 1864; but its visits are casual there and at Singapore, and we have no account of the disease having regained its footing and become endemic. Considering the frequent outbreaks of cholera among our troops in China,* and among the French at Saigong, it is only surprising that there is not more cholera at Singapore. The Island of Ceylon may be treated as a part of India in which the disease occurs occasionally sporadically. Epidemics are by no means very uncommon; but it is strange that, notwithstanding this being the case, and its ceaseless intercourse with Calcutta, Bombay, and Madras, the disease is scarcely established at Galle as an endemic. Cholera appears to have been introduced by sea into the Mauritius and Bourbon in 1775-76, but to have died away. After 1817 it was introduced again, and there have since then been various epidemics—of late occurring with more frequency, which looks as if the disease might take up its quarters there permanently.

So far, nothing that has been said admits of the faintest controversy; but if we go to another apparently ancient seat of the disease, Arabia—a great deal is disputed, and especially the introduction of cholera, and the outbreak at Mecca in 1865, a subject on which I shall not enter. There can be no question as to the immense extent to which cholera has prevailed in Persia and Arabia since 1821. It was, no doubt, first introduced by sailing vessels; how far it has found a congenial soil in these countries, or how far it is kept up by fresh importations from India, I shall not inquire; but these are the general facts, that Surat and Bombay are the two nearest points by sea to Muscat, and that the shorter a voyage is, the more readily is cholera conveyed. The intercourse between Bombay and the Persian Gulf, the Arabian coast and Red Sea, has been constant. Cholera was as early as 1821 carried to Muscat, and in that year it extended up both sides of the Persian Gulf to Bussora. In 1823 it was in various places on the Arab coast. In 1831 it was in Muscat again, and, according to Clot bey, it was a caravan that carried it from there to Mecca, whence it spread to Suez. In 1835 it was

* The evidence of the existence of true cholera in China in early times appears to me to be insufficient.

in the Hedjaz. It was at Aden (which by the way enjoys a great immunity from the disease) in 1846. In 1853 I know it was all along both sides of the Persian Gulf; in 1858, at Jedda, Loheia Mocca, Hodeiah, Massowa; in 1859, again at Aden, and it can be shewn to have been in some part of Arabia almost every year since that date up to 1865.

The disease is almost always present in the Persian Gulf. I have met with notices of it in 1821, 22, 23, 29, 30, 41, 42, 43, 46, 51, 52, 53, 55, 56, 57, 58, 60, 61, but it would be tedious to examine in detail the diffusion of the disease in Persia. It is often there, and in this year (1868), the Russians report that it has reached Astrabad.

These facts respecting the diffusion of cholera seem to point out some conclusions, which I can only enumerate:—

a. It would thus appear that Arabia and Persia have been visited by cholera for the last forty-six years. In 1854 Heslop wrote from Bagdad that cholera had lingered in the towns in the south long after the epidemic had passed away, giving rise to the opinion that it may now have taken up its abode there constantly, and it has been already well pointed out by the *British Medical Journal*, that there are many parts of India that have not been nearly so often visited by it. However, the Constantinople Conference, having the latest and, it is to be presumed, the fullest and best information, were of opinion that cholera was not yet naturalised in any country out of India.

b. In these days ships, caravans, and pilgrims have undoubtedly aided materially in the spread of cholera. I shall say a word about caravans. We have been in the habit of looking on caravans as great conveyers of cholera. A Persian caravan was said to have carried it to Aleppo. Damascus pilgrims had the credit of carrying it to Mecca, as also caravans from Muscat. It was also said that caravans carried it from Mecca; but we are told now that there is some mistake in this, at least as to the transport of the disease across deserts, for the Conference lays down that "Deserts form a great barrier to cholera, and caravans never imported it by the desert into Egypt and Syria."

(*c.*) This is an important conclusion, if it is to be generally accepted; I have no experience in such matters, but it goes against much that I have read on the subject, and Professor Griesinger told me, that though he has no detailed notes on the subject, he is confident that on one occasion, while he was in Egypt, the caravan coming through the

western deserts of Africa introduced the disease into that country.

d. Before leaving this portion of the subject, a word may be said on the imperfectness of our information. It may be matter of surprise that there are no accounts of cholera in Arabia or in Java after the time of Zacutus and of Bontius in 1632 till the year 1820. But it is by no means certain that the subject has been fully examined. Careful research may bring out more information respecting the disease in those countries as well as in India. It is surprising how very seldom historians, especially Eastern ones, have thought it necessary to specify the nature of plagues or epidemics. Even travellers like Bernier and Tavernier, writing about the same period as De Thevenot and Dellon, do not mention cholera. In India it would appear that there is only one doubtful notice of the disease by a native historian, Kafee Kahn, in 1689. In that country only one notice of it by Paxman has been as yet found between 1709 and 1756, a period of nearly fifty years. For a long time Dr. Paiseley's letter, written in 1774, was considered to be the first account of the disease in India. For earlier accounts within the European period, Indian writers had to go to Java. If the very intelligent authors of the Bengal and Madras Reports on Cholera were satisfied that the disease was a very old one in India, on the authority only of vague Sanscrit accounts and of a foreigner, Bontius, surely the early notices of the disease in that part of the world by Rhazes, Avicenna, and other Arabian writers, along with the positive statement of Zacutus in 1632, afford sufficient presumption that the disease was not unknown in Arabia, although its history in the intervening period has not yet been traced.

Some of the results of our historical and geographical survey, bearing on questions discussed before the Constantinople Conference, are the following :—

1. Whatever importance we may assign to the great outbreak of cholera in Bengal in 1817, cholera still prevails in its oldest seats, and on the side of India in closest and nearest communication with the Persian Gulf and the Red Sea. The common notion that cholera is the product of the low plains of India appears to be groundless, and the importance attached to the Gangetic valley is quite exaggerated. The French idea of uprooting cholera by draining the delta, and as one gentleman put it, by running about twenty canals through it, can scarcely be entertained by any sane mind.

Supposing the delta of the Ganges to be improved, and cholera lessened, one source of the disease would be diminished; but could we imagine that this would have much influence on the other parts of India where it flourishes independently? The idea expressed in some of the latest French writings, that cholera is the result, not merely of English neglect of public works, but of the mental and physical decay, and of the cerebral degeneracy of the natives of India, consequent on English rule and *exploitation mercantile*, is absolutely too absurd for serious consideration.

2. I have not been able to go far into the question, but as far as I can judge, cholera has shown little tendency to spread through Guzerat, Scinde, or Belochistan, by the interior: it has spread chiefly along the coast. Cholera does not seem much inclined to travel through the Punjab, and by Cabul into Persia. Curiously enough, many believe that by this route it came into India in 1844 from Central Asia. The greatest risk, therefore, of its spreading towards Europe is by the Red Sea and still more by the Persian Gulf. Cholera appears to be almost constantly present in the latter. To have any effective quarantine, it must include the Red Sea, the Persian Gulf, and the coast of Arabia. Even if I attached much value to quarantines at all under ordinary circumstances, still one is not presumptuous in saying that a quarantine of the Red Sea alone is useless, and that so extensive a quarantine as would be required, is utterly impracticable.

3. Although I think it quite vain to search for new conditions to account for the outbreak of the disease in 1817, seeing that it did not then show any one feature that had not been often present before—still the inquiry why India should so long have been the seat of the worst form of cholera is a very interesting one, though scarcely to be taken up at the end of a paper, but I may say a word or two respecting a speciality of the habits of the natives of India, which has been considered to favour the development of the disease. It is certainly worth alluding to in these days, when the doctrine is so prevalent that cholera is propagated mainly by the excreta, and when many most intelligent medical officers attribute the fatal prevalence of the disease of late years among European troops to increased use of common privies; and to the growth of cesspools near barracks.

Orton, the author of an excellent work on cholera, wrote thus in 1832. After noticing the risk from persons labour-

ing under choleraic diarrhœa going about unsuspected, he says, "The rapid diffusion of the cholera over Paris has probably arisen from the practice of using public privies, and, among the lower orders, of obeying the calls of nature in the open air. Cholera has committed most extensive ravages amongst bodies of troops in India in the course of two or three days, but by means of the common privies every man of a corps may be exposed in the common privy to a full dose of infection arising from a single individual in the course of a day or two: and among the civil population it is scarcely less evident, for nearly the whole of them pass their evacuations *sub dio*, assembling in great numbers in particular places, where they sit for a considerable time, luxuriously smoking their hookahs."

Orton, therefore, viewed both the scattering of the excreta over the soil, and their collection in privies, as favouring the propagation of cholera, and in this he is followed by many. But in modern times it is usual to draw a distinction between the excretions in their recent state, and after they have undergone fermentation. In the former state they are said to be innocuous, in the latter, to contain the seeds of cholera. I apprehend that no such distinction can be rigidly made. However, I shall merely remark that, although the natives of different parts of India do not differ from each other in their habits in this respect, yet, undoubtedly, the disease has its favourite districts. The great manifestation of the malady in 1817, since which period no one has ever discovered any change in its character, took place long before the adoption of public privies in our chief Indian cities. But it would be beyond the scope of this paper to discuss this or other more or less theoretical questions at any length. My object has been to present simple facts.

VI. The historical facts which we have been examining, may be conveniently summed up as follows:—

1. Malignant cholera appears to have shown itself in one of the first campaigns of Europeans in India in the year 1503. It is a disease which the Portuguese found in India.

2. The first undoubted great epidemic of cholera within the period of European intercourse with India, took place at Goa, in 1543.

3. From the accounts of Zacutus, Bontius, and others, cholera appears about 1632 to have been widely diffused in Java, in India, in Arabia, and in Morocco.

4. In the present state of our knowledge it would appear, that there was a period of quiescence of the disease in the

early part of the 18th century—then a great outburst after 1756, which lasted about thirty years, and was followed by a period of comparative quiescence till 1817.

5. Since 1817, it can scarcely be said that there has been any true period of quiescence of the disease—nor can it be said, that at the present day is cholera unknown in any one of the districts, in which we have the earliest accounts of it.

6. Without following the disease to China and the east, or to Europe and the west, since 1821, cholera has been found to prevail nearly every year in some part of Persia, or of the Persian Gulf, very constantly in Arabia, and occasionally in the Red Sea.

7. Extravagant importance has been attached to the Gangetic valley as a generator of cholera, whereas from the earliest date up to the present day, cholera has existed in other parts of India—quite independently of Lower Bengal, and it so happens that historically, Bengal is the part of India in which no accounts of the very early prevalence of the disease have been discovered.

It may be convenient to present some of the facts contained in this paper in a tabular form.

VII. APPENDIX.

A.

NOTICES OF MORSHI, MORDESHIN, MORT DE CHIEN OR CHOLERA MORBUS, BEFORE 1817, CHRONOLOGICALLY ARRANGED.

Calicut	1503	Lendes da India, by Gasper Correa
Goa (great epidemic)	1543	
Goa	1563	Garcia D'Orta
Goa	1580	A, Costa
Goa	1589	Linschot
Java	1629	Bontius
India, Arabia, and Morocco ...	1632	Zacutus Lusitanus
Goa	1638	Mandelsloe
Boorhampore to Surat	1666	De Thevenot
Surat	1674	Dr. Fryer
Common in Western India ...	1676	Dellon
Madura and Coromandel coast ...	1703	Père Martin
Mentioned as a disease in Bengal	1709	Père Papien
Mentioned as a disease of India	1736	Paxmann
Tinnevelly	1747	Orme
First campaign in Madras country	1756 ?	Dr. Paiseley
Near Pondicherry	1769-71	Sonnerat
Amberpet and Arcot	"	Madras Report
Bombay	1772	Clarke
Trincomalee ... ... before	1774	Dr. Paiseley
Madras	1774	Dr. Paiseley
Mauritius	1775	Dr. Burke
Malabar Coast	1778	Fra Bartolomeo
Coromandel Coast (bad epidemic)	1776-8 ?	Sonnerat
In East Indies	1780	Lind
Ganjam Calcutta Sylhet (epidemic)	1781	W. Hastings, Lindsay, Dr. C. Wilkins, Jameson
Madras, Trincomalee	1782	Curtis, Girdlestone.
Tranquebar	"	Konig
Malabar Coast (epidemic) ...	"	Fra Bartolomeo
Bombay	"	Clarke
Along the whole Madras Coast ...	1783	Madras Report
In Travancore country	"	Hay, M. R.
Great outbreak at Hurdwar ...	"	Bengal Report
Vellore and Arcot	1787, 8, 9	Madras Report
Ganjam	1790	Bengal Report
Malabar Coast ... ever since	"	Dr. Macrae.
Travancore	1792?	Hay, M. R.
Backergunge	1797	Taylor, Topog. of Dacca
Trincomalee	1804	J. Johnson
At various stations in Bengal ...	1808, 9, 11, 12, 13, 14	Records of Bengal Medical Board
Jaulnah	1814	Madras Report
Purneah	1816	Calcutta newspaper in 1831
Kishnaghur and Mymensing May and June	1817	Bengal Report
Jessore ... ... August	"	" "

NOTICES OF CHOLERA NOT FULLY VERIFIED.

Aurungzebe's army at Beejapore	1689	Kafee Khan
Nadir Shah's army	1735	Mere native reports

Epidemic, with sudden deaths near Tinnivelly	1747	Orme, vol. ii, p. 201
Arcot	1756-7	French writers
Bengal	1762	French writers, disproved from Lind
Bundlekund	1780	Bengal Report
General André's army	1781	French writers
Mahratta country	1790	Mr. Tuke in Bombay Report

B.

SOME EASTERN NAMES FOR CHOLERA.

Arabic	Almaida, haiza, maradi eswed, saida
Sanscrit	Visuchika,* Chŭrdiê Rogum
Mahratta	Mŏdshi, or provincial modavasi; corrupted mordshi, mordeshin, mort de chien, tural
Malabaree	Niritiripa, nicomben, shani
Concan	Vait
Guzeratee ...	Hagok, koganla
Cashmeeree ...	Dakee
Deccanee	Dānk lugnā
Hindostanee ...	Upurwā i tarwā i, said to have been called mirgee at Bombay
Bengalee	Olă ŭtha
Tamul	Ennerum vandi
Teloogoo	Vantee

* This word has been translated by Hay as meaning "bad air," but I am told it means only an affection of the bowels.

ON THE OUTBREAK OF CHOLERA AT THEYDON BOIS, ESSEX, IN 1865,

WITH SPECIAL REFERENCE TO THE PROPAGATION OF CHOLERA BY WATER AS A MEDIUM.

BY J. NETTEN RADCLIFFE.

[*Read April 1st*, 1867.]

I PROPOSE to lay before the Society a detailed history of the outbreak of cholera at Theydon Bois, Essex, in the autumn of 1865. This outbreak furnishes the most complete, because least doubted and doubtful, illustration with which I am acquainted of the propagation of cholera by means of the drinking water. It is of interest also because during its progress other and more obscure agencies than water came into play in propagating the disease. The entire series of facts, indeed, afford data of the highest moment in elucidating some of the phenomena of the dissemination of cholera. I investigated the circumstances attending the outbreak, at the time of its occurrence, for the Medical Department of the Privy Council; and I proceed now to recount them in the order in which they were ascertained. By adopting this course I shall most clearly set forth the facts and show their significance.

The outbreak took place at a farm known as Little Gregory's Farm, about a mile and a half distant in a south-east direction from the town of Epping.* This farm is situated in Theydon Bois, a parish of the Epping Union, and, according to the last census, containing 125 inhabited houses and a population of 610 souls. The farm was occupied by Mr. William Groombridge, a brick-maker as well as farmer, and the house in which the family resided and a cottage attached to the farm and brick yards are the only dwelling-houses on the premises. These houses are quite isolated from others in the parish, the nearest inhabited building being fully a quarter of a mile distant. At the time of the outbreak, the inmates of Mr. Groombridge's house were twelve in number, namely, Mr. Groombridge, his wife, four daughters, two sons, Mrs. Groombridge's mother, a visitor (female), a maid-servant, and a servant-boy.

Mr. and Mrs. Groombridge had visited Weymouth, and remained there seventeen days. They returned home on

* The hill upon which the farm-house is built is marked "Piercing Hill" in the Ordnance Map of the district.

the 25th September, travelling by the London and South-Western Railway direct from Weymouth to London.

On the evening of the 23rd September, while still at Weymouth, Mr. Groombridge was seized with diarrhœa, sickness, and cramps. The diarrhœa and cramps continued more or less throughout the 24th. Towards the close of that day he took a strong dose of cayenne pepper, obtained relief, and the next morning, although not well, felt able to travel. He did not seek for medical aid in this attack, although a medical man was living under the same roof. In the course of a subsequent and fatal seizure at home, he remarked that the diarrhœa, sickness, and cramps he had suffered from at Weymouth were as violent as he then experienced.

Mrs. Groombridge (æt. fifty years) during the journey from Weymouth was indisposed. She complained of pain in the back and some discomfort of the stomach and bowels, which she attributed to the shaking of the train. On the 26th September, the day after her return home, she was attacked with diarrhœa. On the 27th the diarrhœa persisted, and there was occasional sickness. On the 28th the sickness and purging became violent, and were accompanied by cramps. The matters ejected assumed a "rice-water" appearance, and the prostration was alarming. On the 27th Mrs. Groombridge, after a transitory rally in the morning, passed into a state of collapse, the surface being blue and cold, the eyes deeply sunk in the sockets, the extremities shrivelled, the tongue cold to the touch, the pulse almost, if not altogether, imperceptible at the wrist, and the urine entirely suppressed. On the 30th reaction commenced. This was followed by secondary fever, and she died on the 11th October.

Mr. McNab, Senior, of Epping, the family medical attendant, was sent for, and prescribed for Mrs. Groombridge on the 26th.

Early on the morning of the 30th September, Miss Emily Groombridge, eight years old, complained of sickness and headache. About noon she was attacked with vomiting and purging, the evacuations having a "rice-water" aspect. Profound collapse quickly followed, and she died the same night shortly after 7 o'clock.

During the night of the 30th, James Bass, seventeen years old, a youth in the service of the family, and sleeping in the house, was seized with violent sickness and purging. There had been previous diarrhœa of several hours' continuance. The evacuations were of a "rice-water" character. There were severe cramps, great prostration, and suppression of

urine. He rallied slowly and suffered from consecutive fever, but eventually recovered.

On the 2nd October, Mr. McNab, aged seventy-four years, the medical attendant of the family, after a very brief period of slight relaxation of the bowels, was suddenly seized, about 9 p.m., with copious "rice-water" evacuations, violent sickness, and severe and prolonged cramps. Profound collapse rapidly supervened, the tongue and extremities being shrivelled, the tongue remarkably so, and ice-like to the touch, while the surface generally was clammy and of a death-like coldness. Death occurred within ten hours.

On the 2nd October also Miss Kate Groombridge, aged sixteen years, was troubled with a slight diarrhœa. In the evening there was some sickness. On the morning of the 3rd the diarrhœa and sickness rapidly increased, and were accompanied by severe cramps. The ejections from the mouth and anus had a rice-water aspect. The surface was livid and, together with the tongue, icy to the touch. The extremities were shrunken, the features pinched, the eyes injected and deeply sunk in their sockets. The wrist was pulseless, the urine suppressed, the thirst insatiable. In the evening there was a rally. Reaction set in, and was followed by secondary fever. The functions of the kidney returned slowly. She lay in a precarious condition several days, but in the end recovered.

On the 5th October the maid-servant, aged eighteen years, suffered from diarrhœa all day. On the 6th she was attacked with sickness and cramps, and the alvine evacuations became more copious, thin and colourless like water. There was slight prostration only, and on the 7th she left her bed. On the 8th she had a relapse. The vomiting, purging, and cramps returned with great violence, and the evacuations were of a "rice-water" character. She again rallied rapidly, and left her bed on the 11th. The bowels remained irritable, she was stupid and heavy, and did not recover her strength. On the 13th she complained of "pain in the bones" and head, and she had paroxysms of shivering alternating with flushes. A scybalous motion was passed. On the 14th she was unable to leave her bed, and the surface was cool and covered with a clammy perspiration. The bowels again became relaxed. On the 15th she was delirious; there was occasional sickness, and the whole of the abdomen was tender to the touch and tympanitic. The prostration gradually increased; the distension of the abdomen augmented; the diarrhœa continued; sordes accumulated on the teeth; the tongue was black and dry in the centre, glazed at the edges,

and the tip was like a "nutmeg-grater." There was no eruption. She died early on the morning of the 19th.

On the 5th October, John Riley, aged thirty-four years, a labourer working on the premises, and passing backwards and forwards to the house, was seized with diarrhœa. The bowels continued disturbed on the 6th. On the 7th, cramps and vomiting, and purging of a "rice-water"-like fluid supervened. Collapse rapidly followed, and he died, at his own residence, on Bell-common, the same day at 11.30 p.m.

On the 6th October, at 4 p.m. Mr. Groombridge, aged forty-nine years, was suddenly seized with violent sickness, purging, and cramps. From the beginning of the attack the evacuations from the stomach and bowels had a "rice-water" appearance. The deepest collapse quickly followed, and he died at 7 o'clock the next morning. In the interval between the seizure at Weymouth and the fatal attack the bowels had always been more or less relaxed.

On the 6th October also, Mr. Charles Groombridge, aged twenty years, was seized with diarrhœa. On the 7th severe cramps set in with profuse vomiting and purging of a "rice water" like fluid, and deep collapse followed. The surface was livid and of a death-like coldness. The wrist pulseless; the urine suppressed. On the 8th he rallied. Reaction was slow but continuous. On the 11th he was out of danger. There was no relapse and no secondary fever.

On the 6th October also Mrs. Parcell, aged eighty-seven years, mother of Mrs. Groombridge, was seized with diarrhœa. On the 7th she suffered from sickness, cramps, and purging of a clear watery fluid. On the 8th the sickness and purging abated, but there was much prostration and restlessness. During the 9th and 10th the stools became feculent although still very liquid. Subsequently there was tenesmus, and blood was passed with the motions. The patient, previously somewhat fatuous, refused food. The exhaustion steadily increased, and she died on the 14th.

On the 10th October, Susan Saville, aged forty-six years, the mother of a large family, and residing near the toll-bar, was seized about 8.30 a.m. with purging, sickness, and cramps. She had assisted on the 8th, although slightly, in laying out the body of John Riley. Neither the purging nor the vomiting was profuse, but the matter ejected quickly assumed a "rice-water" character. The stomach was excessively irritable, and rejected almost everything. As the attack advanced the patient became very deaf for a short period. The extremities were cold and covered with a clammy sweat, and the hands and arms were blue and

shrivelled like an anatomical preparation, but the trunk throughout retained some portion of its warmth, and the face did not acquire the choleraic aspect. Towards the close of the case there were two evacuations from the bowels, of a dirty coffee-coloured appearance and very offensive. She was suffered to rise from her bed to pass these motions. Much exhaustion followed, and she died suddenly at 1 o'clock p.m. on the 11th.

Susan Saville was nursed by a married daughter, named Hagger, who lived in a neighbouring cottage, to the east of the "Bell" public-house. On the 31st October, a male child of this daughter was seized suddenly, shortly after 7 o'clock p.m., with vomiting and purging of a "rice-water"-like fluid. Death followed in thirty hours.* Another child, aged six years, was seized with diarrhœa on the 1st November, but recovered.

In addition to the foregoing cases, it must be noted that of the remaining members of Mr. Groombridge's family, Miss Groombridge, the oldest daughter, who principally nursed her father and mother, suffered more or less from diarrhœa for several days after the 4th October. Miss Sarah Groombridge, the second daughter, also suffered during the first week of October from nervous excitement with occasional sickness and flatulence. Miss Rumbold, a visitor, was affected about the same period with like symptoms. The youngest son, aged twelve years, the only other member of the family residing in the house, did not show any signs of indisposition.

A foreman of works, named Todd, who assisted in Mr. Groombridge's sick-room after the death of his employer, had considerable disturbance of the stomach without diarrhœa for three or four days; but this was assigned to accidental causes.

For the preceding histories I am chiefly indebted to Mr. Joseph Clegg and Mr. McNab, Junior, of Epping, surgeons, who courteously afforded me every assistance in carrying out my inquiry. Mr. Clegg had attended the different members of the Groombridge family after the death of Mr. McNab, Senior. He also attended John Riley and Susan Saville's grandchild. Mr. McNab, Junior, had the charge of his father's case, also of the case of Susan Saville.

The following is a summary of the marked cases of choleraic disease:—(*See Table* p. 91).

* Mr. Clegg has ascertained that the child's mother, for fear of conveying contagion, did not communicate with her family after her mother's death, until two or three days before the child was seized.

The details which have been given show that in this outbreak, from the 26th September to the 31st October inclusive, twelve persons were attacked with a diarrhœal disease which, with perhaps one exception, in symptoms, progress, and fatality could not be distinguished from true epidemic cholera. Eleven of the individuals were attacked within the first fourteen days of the period, and of these eight lived in Mr. Groombridge's house. Two of the remainder (Mr. McNab and John Riley) were in constant communication with the house and family, or sick at the beginning of the outbreak. The eleventh person attacked took part in laying out the dead body of the tenth, and the twelfth was a grandchild of the eleventh. Of the twelve cases nine died.

An inquiry directed to ascertain the amount and character of diarrhœa in Theydon Bois, and subsequently extended to other parishes of the Epping Union, proved that there was no unusual prevalence of the disorder among the population, and that the cases occurring differed in no respect from those observed ordinarily in autumn. Two or three cases of common sporadic cholera had taken place in Epping, and several in the Harlow and Matchingham district about the time of the outbreak. But none of these cases proved fatal or presented any peculiar symptom. During the second week of the outbreak the applications to Mr. Clegg for diarrhœa medicine from Theydon Bois and the adjoining parish of Theydon Garnon were very numerous; but this was probably owing to the energetic measures which that gentleman had taken, to impress upon the inhabitants the importance of applying early for medical relief, if the bowels should become relaxed.

The remarkable limitation of the choleraic outbreak to one family and its immediate dependents, with two exceptions, and to one house, indicated some special localising cause. This was not difficult of discovery.

The house in which the outbreak happened is a roomy brick habitation, built about six years ago, and placed upon the summit of a hill. It is fully exposed in all directions, and commands charming stretches of well-wooded and highly cultivated country. There is excellent cellarage beneath; the walls immediately above the surface and within the cellars are dry, and the foundation is sunk altogether in a bed of stiff brick-clay. The entire drainage of the house is conducted into a pond about 100 yards to the E.N.E., and below the level of the foundation. Another pond, also below the level of the foundation, is situated to the

SUMMARY.

	Residence.	Name.	Age.	Date of attack.		Death.		Recovery.	
				Diarrhœa.	Cholera.	During collapse.	During consecutive fever.	From collapse.	From consecutive fever.
I.	Little Gregory's Farm	Mrs. Groombridge	50 years	Sept. 26	Sept. 28	...	Oct. 11	...	...
	"	Miss Emily Groombridge	8 years		Sept. 30	Sept. 30	...	...	...
	"	James Bass	17 years	Sept. 30	Oct. 1	...	...	...	Oct. 14
	"	Miss Kate Groombridge	16 years	Oct. 2	Oct. 3	...	...	...	Oct. 17
	"	Maid-servant	18 years	Oct. 5	Oct. 6	...	...	...	...
					Relapse, Oct. 8		Oct. 19		
	"	Mr. Groombridge	49 years	Sept. 23	Oct. 6	Oct. 7	...	...	...
	"	Mr. Charles Groombridge	20 years	Oct. 6	Oct. 7	...	...	Oct. 11	...
	"	Mrs. Parcell	87 years	Oct. 6	Oct. 7	Oct. 14 Exhaustion	...	...	...
II.	The Place, Epping	Mr. McNab	74 years	Oct. 2	Oct. 2	Oct. 2	...	...	...
III.	Bell Common	John Riley	34 years	Oct. 5	Oct. 7	Oct. 7	...	...	...
IV.	Cottage near the toll-bar	Susan Saville	46 years	...	Oct. 10	Oct. 11 Syncope?	...	...	...
V.	Cottage near the Bell public-house, and a little to the east of the toll-bar	Henry Hagger	3 years	...	Oct. 31	Nov. 1	...	...	...

E.S.E. In the rear of the latter pond is a farm-yard, while further away in the same direction is the brick-yard. Occasionally, when the wind blew across the first-named pond (the cess-pool) and into the open mouth of the common house sewer, offensive effluvia would be forced back along the drains into the house. Sometimes, also, in certain states of the atmosphere there would be an influx of sewer air. But these evils were rarely experienced,* and in other respects, with one prominent exception, the hygienic condition of the house and vicinity was unusually good.

When Mr. McNab had first visited Mrs. Groombridge he had inquired respecting the state of the water used for domestic purposes. This was obtained by means of a pump from a well beneath the floor of the scullery. The well is sunk eighteen feet in the clay, and is supplied with water from an opening about two and a half feet from the bottom. This opening communicates with the pond E.S.E. of the house, the water of which is entirely derived from surface drainage. Water was fresh drawn from the well for Mr. McNab's inspection. It was clear and bright, and neither in smell nor taste (the latter tested by letting some drops fall from a finger† upon the tongue) seemed objectionable. When after Mr. McNab's death Mr. Clegg took charge of the sick, he finding the mischief still increasing among the family without any obvious cause, and having a well-founded doubt of the purity of the water supply of the entire neighbourhood, at once stopped the use of the water from the house pump. The wisdom of this course will be apparent in the sequel. It probably prevented the entire family being swept off.

When I first visited the house, the 11th of October, the pump had been disused for seven days. On setting the pump again in action, the water poured out proved to be very turbid and abominably fetid. The odour of sulphuretted hydrogen was conspicuous. It was clear that the water was laden with decomposing organic matter. The source of pollution of the well was sought for on the 13th, and on

* So I was told. But a privy in the rear of the house communicated with the common sewer. If this privy were regularly flushed, and the sewer kept clear, the atmosphere within the house would probably suffer only as stated. If it were not flushed regularly, the drain would become simply an elongated cesspool, and there would be a constant diffusion of sewer air into the house, while the partially blocked drain would augment the evil about to be described, by preventing a free flow of sewage matter.

† Mr. McNab, Mr. Clegg states, used the right hand, the same hand with which he had a few moments before examined two of the cholera cases.

this day I collected specimens of the water and forwarded them for analysis to Professor Miller of King's College. He found traces of sulphuretted hydrogen, and a very large amount of ammonia; there was also a quantity of organic matter in suspension. Free oxygen and nitrates were absent. "There was obviously," he remarked, in his report, "some leakage from the cesspool into the well."

The termination of the soil-pipe of a water-closet on the first floor and the commencement of a sink-drain, each lay within a yard of the mouth of the well. On opening the well indications of percolation through the brick-work nearest the sink, and at a little distance below the level of the mouth, were at once perceived. On pouring water down the sink-drain a steady filtration at the suspected point soon commenced, showing that a communication existed between the sink-drain and the well. On removing the brick-work where the filtration occurred, the soil at two feet six inches from the surface, and in the angle near the pump, was found to be saturated with drainage matter. But a careful examition failed to discover any leakage, either from the sink-drain or the soil-pipe inside the walls. On prosecuting the search outside, however, it was discovered that the continuation of the sink-drain and the drain in which the soil-pipe terminated (the one drain not having any communication with the other) were defective, and that from both a leakage took place. The escaped matters penetrated downwards along the outer wall of the house, passed beneath the foundation, saturated the earth in the angle between the pump and the well, and so reached the latter. Water having been poured down the water-closet, in ten minutes a portion had passed along this tract and was dripping into the well. The soil-pipe from the water-closet was constructed of ordinary glazed stone-work until it passed beyond the external wall of the house, when it was continued by common six-inch unglazed butt-pipes without sockets, and simply placed end to end. The leakage took place at the first joint between the glazed and unglazed pipes, and the porous material of the latter permitted free percolation of liquids.

It would appear to have been thought in the building of the house that the interposition of the thick and solid foundation wall between the drains and the well would be a sufficient safeguard against contamination of the latter by leakage and soakage. The error is one liable to occur; and instances of the pollution of house-wells by soakage from without beneath foundation walls are, I believe, not common. Hence I have not hesitated to describe the source of pollu-

tion in the present case at somewhat greater length than might else have been necessary.

The contamination of the well-water was not of recent date. Early in June, Mr. Groombridge had consulted Mr. Forshall, surgeon, of 16, Beauvoir Terrace, Kingsland Road, N.E. He was then suffering from obstinate indigestion, irregularity of bowels, particularly a desire to go to stool immediately after taking food, inaptitude for active exertion and great depression of spirits (although his business affairs were prosperous). His health had been more or less disordered for several months. As no relief was obtained from the medicines prescribed, and Mr. Forshall was informed that Mrs. Groombridge and Mrs. Parcell were suffering in a less degree from similar symptoms, he suspected that the water used by the family might be impure. In answer to his questions Mr. Groombridge stated that it was very hard and gave to tea a disagreeable flavour. Mr. Forshall requested that a specimen should be brought to him. This he received on the 17th of June. The water was limpid, but it had an unpleasant odour and nauseous taste, and it was submitted to Mr. Goff, of the Kingsland Road, an operative chemist, for analysis. Mr. Goff reported that the specimen contained traces of sulphuretted hydrogen and much organic matter. Notwithstanding this discovery, the faith of the family in the goodness of the water was not shaken, and they persisted in its use until the outbreak of cholera.

Mr. Forshall prescribed both for Mrs. Groombridge and her mother; and it was upon his recommendation that Mr. and Mrs. Groombridge visited Weymouth.

From Mr. Forshall's statement it is apparent that Mr. Groombridge's family had been drinking water contaminated by drainage and surface impurities from early in June, probably for a longer period. The outbreak, which is the immediate subject under consideration, may be surmised to have followed upon the further pollution of the water by the alvine evacuations of Mr. and Mrs. Groombridge during the two or three days immediately following their return from Weymouth. At this time the first-floor water-closet was in frequent and unaccustomed use; large quantities of liquid fæces were received in it, and all the ejections both from the stomach and bowels, during the first brunt of Mrs. Groombridge's illness, were cast down it. The largely increased quantity of water passing from the water-closet along the soil pipe and terminal drain would contribute to a more rapid contamination of the well.

It was important to ascertain if Mr. and Mrs. Groombridge had been exposed to any specific choleraic infection whilst at Weymouth. To investigate this question I visited Weymouth, examined the house in which they had lodged and the persons who had known their habits while there, and made a house-to-house visitation of the immediate neighbourhood without discovering anything that could throw light upon the early origin of Mrs. Groombridge's attack. I had interviews with all the medical men but one in Weymouth and Melcombe Regis. All concurred in the statements that no case approaching in character to epidemic cholera, or choleraic diarrhœa, or autumnal cholera, had been observed in that town and district during the month of September, and that autumnal diarrhœa had been less prevalent than usual. I afterwards visited Portland and Dorchester, both of which places had been visited by Mr. and Mrs. Groombridge, and ascertained from the medical practitioners that both places had been equally free from choleraic disease.* I then directed my attention to Southampton. Mr. and Mrs. Groombridge travelled from Weymouth to London by the early morning express train on the London and South-Western Railway, passing through Southampton West in their transit. It was necessary to inquire if they had been exposed to infection at Southampton, where epidemic cholera had broken out. They did not stay there, so far as I could ascertain, longer than the stoppage of the train. If they left the train for a few moments, they would probably not leave the railway station. Such chances of infection as they were subjected to there would be such as arose from the brief halt of the train at the station and its rapid passage through a portion of the town. Mr. Groombridge had been attacked with diarrhœa on the 23rd September at Weymouth. The first death from cholera at Southampton occurred on the 24th September, in Brewhouse Court, Brewhouse Lane, near the lower extremity of the High Street, the case dating from the 17th of the month. The second death took place on the 29th September in Anglesea Terrace. In the interval five persons had been attacked in the vicinity of Southampton, three at Weston Common, and two at Bitterne. Both localities are situated two miles from Southampton.

* Mr. Simon, commenting upon this outbreak (*Eighth Report*, p. 28), writes, "accidentally, I am informed, that a gentleman from a distance, who early in August was spending a week in Weymouth, and visiting both Portland and Dorchester, contracted during that week a diarrhœa, which, on his return home, developed to severe cholera."

Brewhouse Court, in which the first case of cholera in Southampton occurred, is about five furlongs distant from the nearest point of the Weymouth Railway, and rather more than five and a half furlongs from the Southampton West station. Anglesea Terrace, where the second case occurred, is at a greater distance from the station, but one-third less distant from the nearest point of the railway. No cholera or choleraic diarrhœa is known to have existed in the vicinity of the railway, where it passes through Southampton, on the 25th September.

At the time of the investigation, the assumed rapid passage of Mr. and Mrs. Groombridge through Southampton, and the position of the localities then infected with cholera, namely, Brewhouse Court and Weston Common, led me to conclude that it was in the highest degree improbable that they contracted cholera there. A more accurate study of the diffusion of epidemic cholera in 1865, together with the facts recorded in the elaborate history of the outbreak of the disease at Southampton in 1865 by Professor Parkes,* has induced me to modify this conclusion. Assuming that the diarrhœal disease at Theydon Bois was true epidemic cholera, an assumption which seems to me almost absolute from the occurrence of consecutive fever, and the transmissibility of the disease from the sick to the well, I am disposed to think that there is a flaw in the history of the first case.† Evidence necessary to the complete history of the outbreak probably died with Mr. and Mrs. Groombridge.

As the facts stand they may be thus summed up:—Mr. Groombridge returned from Weymouth with relaxed bowels. Mrs. Groombridge was attacked with diarrhœa a few hours after she reached home. In both cases the diarrhœa was probably perpetuated and exaggerated by the contaminated water of the house well. Almost from the moment of reaching home the contamination of the water would be augmented by the liquid diarrhœal discharges of Mr. and Mrs. Groombridge passed down the water-closet, and percolating more or less freely into the well, and quickly after by

* In the course of the 23rd September Mr. Groombridge spent some time on the hills overlooking Portland Harbour. The wind was blowing strongly from the sea, and he noticed vessels passing in which clothes were hung in the rigging to dry. Mr. Clegg asks if infection might not have been blown from the ill-washed linen of a choleraic patient? I was not able to obtain any information concerning the ships which entered or sailed from Portland Harbour at this time. A return furnished by the Customs' Office respecting Weymouth, showed that no vessel open to suspicion had entered that harbour in August and September. Mr. Groombridge became indisposed while watching the vessels he described.

† Eighth Report of the Medical Officer of the Privy Council.

the alvine evacuations of Mrs. Groombridge. To this special cause of pollution was doubtless owing, first, the choleraic attack of Mrs. Groombridge, and subsequently the seizure of other inmates of the house and of John Riley. All these persons used the water of the contaminated well habitually.

Three cases cannot, however, be included in this category, the cases of Mr. McNab, Susan Saville, and Susan Saville's grandchild. It is not known that Mr. McNab tasted the water otherwise than has been stated. Susan Saville had not at any time drunk of the water; she had never visited Mr. Groombridge's house (a mile distant from her cottage), or had communication with any member of the family during the outbreak, and she had not washed any linen soiled with the discharges of the sick. She had not assisted in nursing John Riley; she solely helped in laying out his corpse. The grandchild's mother had nursed Susan Saville throughout her fatal illness.

The chronological sequence of the cases was as follows:—

Well polluted with diarrhœal discharges, 27th September.

Water from the Well disused, 4th October.

1. Group of Cases determined by the polluted Water.

Order of Succession of Cases.		Diarrhœa after pollution of Well.	Cholera after pollution of Well.	Result.
1st case	Mrs. Groombridge		1st day	Death, 13th day, consecutive fever
2nd case	Miss Emily Groombridge		3rd day	Death, 3rd day, collapse
3rd case	James Bass	3rd day	4th day	Recovered
4th case	Miss Kate Groombridge ...	5th day	6th day	Recovered
5th case	Housemaid	7th day	10th day	Death, 21st day, consecutive fever
7th case	James Riley	7th day	9th day	Death, 9th day, collapse
8th case	Mr. Charles Groombridge	8th day	9th day	Recovered
9th case	Mr. Groombridge.....		8th day	Death, 9th day, collapse
10th case	Mrs. Parcell	8th day	9th day	Death, 16th day, exhaustion

2. Group of Cases determined in some other way.

Order of Succession of Cases.			Result.
6th case	Mr. McNab	5th day after liability to infection	Death, same day, collapse
11th case	Susan Saville	2nd day ditto.....	Death in 29 hours, collapse
12th case	Susan Saville's Grandson	21st day after exposure to indirect infection	Death after 30 hours' illness

The foregoing sequence of cases is in remarkable contrast with that which occurred in the Broad Street (Golden Square) outbreak in 1854. In that instance the outbreak was determined almost demonstrably by the use of water from a particular pump, the water of which had been polluted by the diarrhœal ejections of a child at the time of an epidemic of cholera. The attacks of fatal cholera occurred in the following daily order:—

The Broad Street Outbreak, 1854.

		Cholera. Fatal Attacks.
Child's dejections thrown into cesspool communicating with the well of the pump . . .	August 28th	3
	,, 29th	2
	,, 30th	3
	,, 31st	34
	September 1st	142
	,, 2nd	128
	,, 3rd	62
	,, 4th	55
	,, 5th	26
	,, 6th	28
	,, 7th	22
Pump handle taken away and use of the water stopped	,, 8th	14
	,, 9th	6

Both this outbreak and the outbreak at Theydon Bois furnish invaluable data in estimating the probable effect of water contaminated with diarrhœal discharges in determining explosions of cholera, when the influence of such a cause of contamination has to be estimated chiefly, if not solely, from the facts of the explosion alone.

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REMARKS ON THE OUTBREAK OF CHOLERA IN BROAD STREET, GOLDEN SQUARE, LONDON, IN 1854.

By the Rev. H. WHITEHEAD, M.A.

Read May 6th, 1867.

It is commonly supposed, and sometimes asserted even at meetings of Medical Societies, that the Broad Street outbreak of cholera in 1854 was arrested in mid-career by the closing of the pump in that street. That this is a mistake is sufficiently shown by the following table, which, though incomplete, proves that the outbreak had already reached its climax, and had been steadily on the decline for several days before the pump-handle was removed. Had it been possible to make it complete, the table, as I will presently show, would have pointed still more decisively to the same conclusion.

Meanwhile, to indicate the well-defined and concentrated character of the outbreak, I may here state that during the first thirty days of August there had been an average of but one death per day from cholera throughout the parish (St. James's, Piccadilly) in which Broad Street is situated; that the earliest cases on August 31st did not manifest themselves till about noon on that day, and that nearly all the deaths here recorded occurred within an area of about 250 yards radius from the pump as its centre.

Date.	No. of Fatal Attacks.	Deaths.
August 31st	34	4
September 1st	142	72
„ 2nd	128	127
„ 3rd	62	76
„ 4th	55	71
„ 5th	26	45
„ 6th	28	40
„ 7th	22	34
„ 8th (Pump closed)	14	30
„ 9th	6	24
„ 10th	2	18
„ 11th	3	15
„ 12th	1	7
„ 13th	3	13

„ 14th	0	6
„ 15th	1	8
„ 16th	3	6
„ 17th	4	5
„ 18th	0	4
„ 19th	1	4
„ 20th	0	1
„ 21st	0	0
„ 22nd	2	3
„ 23rd	0	3
„ 24th	1	0
„ 25th	0	1
„ 26th	1	2
„ 27th	0	0
„ 28th	2	2
„ 29th	0	1
„ 30th	0	0
Date unknown	127	46
	668	668

The forty-six deaths of unknown date are stated by the Committee of Inquiry to have nearly all occurred during the first week of September; and, as the omission is due to the confusion which prevailed, they most likely occurred on the first two or three days, when the confusion was such as few who were not in the midst of it can imagine. For instance, on the night of September 2nd there were as many as eighty-two bodies in the "dead-house."

Moreover, as the cases in this table are those of resident inhabitants, there is no account here taken of non-resident work-people who, after being attacked within the "area," died at their own homes out of the district. Their number is reckoned by the Committee as about forty, of which twenty-eight belonged to Broad Street alone; and these twenty-eight were all seized before the evening of September 2nd, after which date the factories were closed for a time.

It is certain, then, that the table, if more complete, would have indicated more clearly even than it now does the exceeding virulence of the outbreak during its two or three earlier days, and the rapidity of its decline after the climax was passed; and the column of fatal attacks, incomplete as it is, establishes the fact that the climax was reached within a very few hours of the first manifestation of the outbreak.

Clearly the original cause of the explosion must very soon have lost much of its fatal power. That the Broad Street well-water was this cause I am not now concerned to show. Provisionally assuming it to have been the cause (and prepared, if necessary, to prove it), I will try to define the limits (as to time) of the fatal operation of the water, and perhaps incidentally to throw some light on the period of "incubation."

I had occasion, as a member of the committee, very closely to investigate all the circumstances of the outbreak as connected with the Street (Broad Street) in which the greatest mortality, 10 per cent ($\frac{90}{890}$), of its resident inhabitants occurred; add to this mortality the cases of the twenty-eight non-resident work-people; add again fifty persons who recovered from severe attacks, most of whom I personally examined, and it will be admitted that the results of a close investigation of such a street would probably fairly represent the results which would have been obtained by a similar examination of the whole area. But I did also, in conjunction with Dr. Snow and others, though not with equal minuteness, pursue the inquiry from day to day for weeks together in other streets, and we found that the results, as far as they went, corresponded.

We found then that, whilst it was exceedingly difficult to disconnect any of the earlier cases from the use of the pump-water, we discovered less and less connexion between this water and the disease on each succeeding day after September 3rd; and among the attacks, fatal and not fatal, after September 6th, we could trace no such connexion at all.

Indeed, in Broad Street itself the outbreak completely ran its course in nine and a half days. I succeeded in ascertaining the alleged *hour* of fatal attack in every case in this street; and I know that no attack, fatal or not fatal, occurred in it after September 9th.

It is worth noticing that generally the streets nearest to the pump became the soonest free from the epidemic. "The suddenness of the principal outburst," say the Committee, "as also its rapid subsidence, is chiefly marked in those streets and courts which are nearest to the centre of the cholera area; whilst in the border of this space, and beyond its limits, there is no such abrupt and extreme rise and fall in the number of attacks."

Thus, during the decline of the outbreak, we seem to catch sight of an outside limit to the fatal operation of the water.

But how much earlier than the 6th, by reason of the incubation time, we are to fix this limit it is of course hard to say. For instance, a friend of mine, the Scripture reader of St. Luke's Church, told me that on the evening of September 2nd he quite exceptionally drank about half a pint of the pump water from the ladle. On the September 4th he began to suffer slightly, and on the 6th severely, from diarrhœa; on which latter day I found him in bed very ill indeed. Now if he had died, and without telling me any of these circumstances, I should certainly have supposed that he was seized on the 6th, as I had seen him at his work every day, and never heard him complain of illness. And as for his drinking of the pump water, if there had been any reason to suspect it at all, I might have thought that he took it later than September 2nd. The value of this case consists in his drinking of this water having been clearly ascertained to be an exceptional act on his part.

From the cases of persons described as having been *habitual* drinkers of this water we can of course learn little to define the exact period of incubation in each case (unless they happened to be seized very early in the outbreak, when by reason of the known duration of the special pollution of the well, of which I will speak presently, we may get at some approximation to the truth); for, especially as this water had to be sent for, even an habitual drinker of it may have omitted to drink it for a day or two, taking the Company's water instead; and so we cannot certainly fix the day on which he first encountered its fatal influence.

Few habitual drinkers of the pump water, to my knowledge, escaped with impunity. Few survivors were able to assure me that they so drank it regularly during the week ending September 2nd. But on and after that day several persons, who had not been in the habit of drinking it, began doing so, at least occasionally, from a notion (due to the enormous quantity of it taken by some patients who revived from collapse) that it was "good for cholera." More of *these* drinkers than of habitual drinkers of the pump-water escaped with impunity, I myself being among the number, as I drank some of it at 11 p.m. on September 3rd, though not from any idea of its beneficial qualities.

Judging from the case of the Scripture reader, we may, perhaps, assign a longer period of incubation to the cases of those who suffered, or even died, among the later than among the earlier drinkers of the Broad Street water.

The proximate cause of the outbreak, as is well known, is

alleged to have been the pollution of the well by the discharges of an infant suffering from diarrhœa, which discharges, diluted with water, began to be poured on August 28th into a cesspool, the communication of which with the well was clearly ascertained. They continued all day on August 29th, and ceased on the 30th, having thus lasted about forty-eight hours.

We have then an interval of four days between the *probable* date of the first pollution of the well and the beginning of the outbreak. But what time must be allowed for the discharges to percolate from the cesspool to the well, and also to become injurious, of course I cannot tell. I only know that we must not ask for any time later than August 30th as the starting-point of the mischief, because one man who exceptionally drank it at noon on that day was seized with cholera, according to his widow's account of the matter, at 9 a.m. on September 1st.

Assuming her to have been right as to the hour of attack, indeed whether right or wrong, we have for incubation time in one case at most forty-four hours. And she was most likely right, because the unanimous testimony of the medical men who were in the thick of the outbreak was to the effect that for the first two or three days there was an almost total absence of "premonitory symptoms."

Still even this interval of forty-four hours is longer than the interval in the few other cases where we can unmistakably isolate the act of the drinking. They are only three in number, and all belong to the 1st of September, on which day, for instance, a gentleman who came up from Brighton in the morning drank the Broad Street water at luncheon in Poland Street, and was seized with cholera in the evening of the following day. We get here about thirty hours for the interval. Another interval under similar circumstances was about twenty-four hours; and another not more than twelve hours.

From these data, considered by themselves, it might be very unsafe to pronounce any opinion as to the day on which the water reached its maximum of special pollution or rather of injurious influence. But, taken in connexion with the general facts of the outbreak and other evidence respecting the effects of the water, they help at least to the conclusion, that the water was not very injurious before August 30th or after September 2nd, on which latter day indeed it seems likely that it was rapidly on its way towards purification.

That the water should so quickly purify itself is not

astonishing when we bear in mind (1) that the discharges ceased on August 30th, (2) that the body of water at any one time in such a well is inconsiderable, and (3) that it was consumed (and therefore replenished) at a most extraordinary rate during the first few days of the outbreak by reason of the almost incredible quantity of it drank by the patients, in some cases as much as seventeen quarts a-day.

It may be seen from the table that, notwithstanding the rapidity of the decline of the outbreak on September 3rd, the rate of decline flagged on subsequent days, so much so as to seem at one time almost to stand still.

This used formerly to perplex me, and I sought an explanation of the phenomenon in the hypothesis of a subsequent pollution of the well by the discharges of other patients (three in number) in the same house on the earlier days of the outbreak, especially as I was told by surviving relatives that the discharges of these patients were carried to the cesspool. But they afterwards confessed to me that they had thrown them out of the windows into the back-yard, the temptation to do so arising from their rooms being at the back of the upper floors. The above-mentioned infant having lain ill in the front kitchen, in this case the cess-pool (being in the front area) was the handiest place of deposit.

Here I must not omit to mention that if the removal of the pump-handle had nothing to do with checking the outbreak which had already run its course, it had probably everything to do with preventing a new outbreak; for the father of the infant, who slept in the same kitchen, was attacked with cholera on the very day (Sept. 8th) on which the pump-handle was removed. There can be no doubt that his discharges found their way into the cesspool, and thence into the well. But, thanks to Dr. Snow, the handle was then gone.

The slackened rate of decline after September 3rd does not, in the present state of knowledge upon the mode of propagation of cholera, need any explanation. I now only wonder that with such a start from the agency of the pump, and with so much material thus supplied for the continunace of the outbreak by other means of communication, it had so little power to sustain itself for any length of time after its original promoter had begun to suspend its fatal operation.

ON THE PREVENTION OF DISEASE BY THE RECONSTRUCTION OF THE DWELLINGS OF THE POOR.

By Dr. T. CLIFFORD ALLBUTT.

[*Read June 2nd*, 1867.]

PREFATORY NOTE.

It is now nearly two years since the present paper was read before the Epidemiological Society. Since that time a great change has come over the state of all large towns—a change which has not left Leeds unaffected. Leeds has been perhaps less depressed by the great ebb of trade than many other towns; but even here numbers of houses are standing empty in streets, where a few years ago a premium was paid for the right of succession. The lower we get in the social scale, the more fluctuating do we find the population; and we find, accordingly, that the demand for low-rented dwellings has fallen off considerably though overcrowding has increased. We have abandoned the idea, therefore, of extending our building operations at present, and many of our present tenements have been unlet from time to time. Like all other owners of cottage property we have also been obliged to lower our rents. I have determined, nevertheless, to print my paper in the *Transactions* as it was read. For even during this unprecedented time of slack trade, we have seen nothing to make us look upon our venture as unprofitable; and, taking the average of times, I am still satisfied that the need of cheap and wholesome dwellings for the very poor is one of the most serious questions of the day.—T. C. A.

It is with feelings of unusual pleasure that I accept the invitation of this learned Society to read a paper upon the dwellings of the poor in populous towns. I honestly think that no more important question can at the present time be discussed.

I have been especially wishful that the medical profession should itself more earnestly take this matter in hand; for it has, I think, been too much neglected by us. While schemes for the improvement of poor dwellings have met with our cordial sympathy, yet we have, I may venture to say, looked at the matter as curious or interested observers and critics rather than as active workers.

Yet surely if our profession is a profession in the highest sense of that word, no work can more especially be ours than work which tends to lessen disease by precautionary measures. It cannot be for our honour to confine ourselves

to the narrower and more immediate aspects of disease, and tacitly to withdraw from any dealing with disease in its larger and more general aspects.

I look, Sir, upon this Society and others of the same kind as protesting against so contracted a view of our callings. The Epidemiological Society proclaims that it is the work of a liberal profession to look first to the public weal. It refuses to regard medical work as insincere work, as work done for trade purposes. In our eyes the medical art is not a trade, nor merely indeed a pleasant and intellectual way of making money; but it acknowledges also a responsibility, a paramount duty of preventing and relieving the physical ills of mankind. This Society has the good fortune to associate with itself, and has the honour of expressing the opinions of an order of medical men, which is destined at no distant time to take a very high, if not the highest, place in our profession,—I mean the Medical Officers of Health. These gentlemen, dealing as they do with the health of communities rather than of individuals, claim thereby a more important place in Society. They are indeed at present awaiting a higher perfection of their own science, a science as full of difficulties as it is full of promise. Labour in that field is, however, winning abundant reward. Scientific industry has seldom seen a more successful issue than the issue of late researches into the origin of the cholera, the cattle plague, and the continued fevers. We are rapidly organising a large body of doctrine on these and like questions, and our Medical Officers of Health having to deal with doctrine at once of such importance to the community, and also of such high scientific generality, having, moreover, great powers to deal with difficult and delicate questions of social and proprietary rights, must be possessed of a learning, an intelligence, and a tact and knowledge of the world which will place them very high indeed among men of any or all professions. The replacement in most towns of under-educated inspectors by highly qualified medical men is one of the best signs of the day. The authorities of any town which as yet remain without the counsels of a medical officer of Health, still venture to incur the awful charge of so neglecting laws which are or may be well known to them, that men, women, and children, whose government and welfare they have undertaken, are allowed to waste in sickness or to die. They deliberately refuse to advise on matters of the life and death of hundreds of people* with those persons who alone possess

* Dr. James Braithwaite has shown that 1790 persons die annually in the Borough of Leeds from preventable causes

the knowledge necessary to save those lives. Is it not a farce, when the authorities of large towns act thus, that we should hang with all the pomp of the law a poor ineffective creature who has but one life at his door. A great change is, nevertheless taking place even in the municipal mind. Upon the urgent representations of the Leeds Sanitary Association; the Council of our borough was persuaded last year to elect a medical officer of health, and since his election I believe that the Council has shown some readiness to give real attention to duties which are, perhaps, the most important it has to perform for the poor. On his arrival the medical officer found 1,500 bedrooms over privies, 500 or 600 cellars used as habitations, a large number of inhabited hovels, which were scarcely fit for dog kennels, and a state of overcrowding naturally to be expected in a town which has increased in number by about forty per cent. during the last twenty years, and which must always have been ill-provided with labourers' houses. For the well-paid artizan cottage houses have sprung up in all directions and with great rapidity. Owing to municipal neglect, however, almost all these are built back to back, and are of the flimsiest material and workmanship. They frequently contain privies under bed-rooms, and in many other cases only avoid this by the simple device of having no privies at all. Still the well-paid artizan does secure something like a home. The unskilled labourer, on the other hand,—the job porter, the hawker, the costermonger, the rug-maker, persons whose livelihood is uncertain, who are improvident or destitute,—these, whose number equals or exceeds the number of the skilled classes, get housed as they can. After prowling about by day after such sustenance as the fowls of the air may bring them, they crowd together by night in dank filthy hovels, huddling in heaps like wild animals to comfort their wretched bodies with a little warmth. What builders will compete for the profit of housing such starvelings as these? What builder will risk his hardly-won capital and the provision for his wife and children on dwellings for such wandering hapless creatures. Instead of returning him the high profits and good security of the better kinds of cottages, such dwellings, if properly built and repaired, offer a low and ill-paid dividend and a low security also. If these, and such as these, and if the class of unskilled labourers just above these are to be housed wholesomely, they cannot be housed at a tempting profit. Low rents, troublesome collecting, and incessant leakages tempt no one.

It is absurd, therefore, in the ears of any one who knows what a large town is, to hear members of Parliament solemnly denouncing all interference with private speculators. No doubt as private builders provide for the skilled artizan, these will in many instances leave worse houses for the new ones, and as they rise the lower working classes may in turn be sucked up into the better homes. I believe this process has but a small place in helping the very poor. In the first place, many of the better houses are still very bad; and, in the second place, private contractors build cottages now because there is an incessant and insatiable demand for them from persons well able to pay, and their profits are therefore great. But the artizans who dwell in these new cottages are, in many cases, importations from other towns, not artizans who have been temporarily resident in worse cottages of the same town. The new cottages represent in Leeds rather the increase of the population than the elevation of it. If a widow woman with five children and her old mother on her hands, live in a cellar, no speculating builder will be tempted to help her out of it. How then are we to act? All persons act ultimately on their own conceptions of their own interests. Have communities any interest in improving the dwellings of their inferior members? Fortunately they have. The neglected poor become, under conditions of foul dwellings, foul air, and foul water, the cause of a vast amount of disease. It has been shown over and over again that the deaths of children under five years of age are almost three times as frequent in the population of crowded courts and narrow alleys as they are among the families of the rich. And how much maimed life does this death-rate represent among the survivors? Again, of the terrible destruction of all sense of decency which results from the indiscriminate housing of the sexes too much is known now to need repetition from me. We have then this state of things to deal with. A community, which must look to a collective life, and cannot exist as a mere aggregation of individuals, has in the midst of its own body a mass of its members subjected to directly poisonous agencies. These agencies are destructive not only to physical health, but to that moral health upon which the vigour and the cohesion of a social body directly depends. This physical and moral mischief is fearfully costly. There is, however, no reason to suppose that private enterprise will be tempted to deal with this state of things for private profit alone.

What, then, is to be the remedy? It must be learnt first

of all, and then well brought home to the bosoms of those who dwell in cities, that this enormous mass of suffering humanity not only lies in the midst of them, but is also of them, enters organically, as they themselves do, into the structure of their particular society. To heal the sores of any part of that social body is, therefore, to heal their own body. To say that the health of the neglected poor is to be left to the natural processes of social growth, is to repeat one of those vague propositions which are at bottom stupid truisms dressed up to look like science. What are the natural processes of social growth? Are they necessarily blind processes like the growth of weeds? Surely there must be room, and more than room, for conscious processes, for the thoughtful adaptation of means to ends, for deliberate interference with the order of change. If in animal life it is the lower modes of growth which are unconscious, and if man, on the contrary, can modify the order of physiological change by conscious and calculated interferences, how much more in the growth of societies shall conscious calculation find a place! We do not refuse to take steps to protect a scrofulous joint on the principle of leaving it to be provided for by the ordinary laws of supply and demand of the nutritive fluids of the body. Did we do so we should allow the joint to become disorganised, and the body to be endangered and destroyed as is the case in savage communities. In the deliberate modification of the order of phenomena by our own reasonable acts consists the superiority of man over other animals, and of highly civilised over barbarous societies. Only in the degree of our power to modify more general laws by special interferences of our own reason can we claim a high place either as individuals or as societies. To abandon our movements, then, to the play of more general laws, without any effort at modification, is to resign all the fruits of civilisation and to fall to the level of savages and shuttlecocks.

If, therefore, social growth in its higher forms must intimately depend upon the awakening of our consciousness as social bodies, upon the development of our reason, and upon elaborate schemes of social action organised by that faculty, we shall see that we are bound to think and consciously to act for other than individual ends. Every citizen must not only drive his own shuttle, but must also look to the weaving of the social web in its larger aspects. He must watch the weal and woe not of himself only, nor only of his family and friends, but also of the community to which he belongs.

"Non nobis solúm nati sumus; ortusque nostri partem parentes, partem amici partem patria vindicant."

It is, then, our bounden duty, among other things, directly or indirectly, to meet by deliberate and reasonable provisions the needs of the ill-housed poor, and to lessen or remove the causes of that misery, sickness, and death which has long dragged heavily upon the vitality of this nation, and is becoming every day a more awful curse.

I need not, Sir, detain you or the members of this Society by a detailed description of the kinds of disease and the kinds of crime which are generated by bad housing. They have been made well known by the labours of this Society and of the Society of Arts; by the labours also of many private persons, among whom I may mention particularly Mr. Torrens, Mr. Waterlow, and Mr. Hole, whose work is now before me. I believe that I shall have no dissentient in this room when I state that for a comparatively small expenditure typhus fever, that great pest of ill-housed towns, and of Leeds in particular, could be made as rare as the plague. How greatly it would diminish other infectious diseases, how greatly this expenditure would prevent the initiation of morbid modes of growth, such as scrofula, rickets, and the like, I cannot tell, but, no doubt, to an astonishing degree. I calculate that an expenditure of £100,000 in the improvement of low-rented houses in Leeds would bring about such a change in that town. I felt, therefore, as Physician to the General Infirmary and to the Fever Hospital of Leeds that I was stultifying myself by standing there with a bottle and a glass, pouring two tablespoonfuls of the mixture as before down the throats of sick men, women, and children, when I was aware that all this disease was being manufactured at one end with an activity that puts to scorn all efforts to cure it at the other. It seemed absurd to stand chopping at the heads of a Hydra, when the serpent itself might be demolished. I calculated, therefore, what expenditure would be needed, and in what way it should be directed, in order to scotch the evil. After many consultations with an architect and a close looking-into things with some of my friends, it seemed that the sum I mentioned above; viz., £100,000, if expended in a certain way, would go very far to meet the needs of the town, and would almost entirely relieve the over-crowded, dark, and filthy quarters. I determined also that it was well at least to begin by building to some extent directly for the poorest classes. I do not think, as I hinted before, that building

for the better class of operatives sets free at present many decent houses for their inferiors. The better houses are built either for new comers or for families rising out of houses which are unfit for any inhabitants. Now what I want to relieve especially are the cellars, cellars where one small room is now taken by a family, which family will often take a lodger likewise.

Unscrupulous and ignorant persons, money-lenders, and agents for unknown owners, secure a shameful profit out of the letting of hovels, cellars, garrets, and such places to the needy poor. When the Council is requested to close such places, they naturally ask whither in that case are the people to go?

I have carefully avoided, Sir, any word-painting about the state of squalor and over-crowding in large towns. This has been done almost to nausea. To give you an idea, however, of the demand for any hole which can be crept into, I may relate an anecdote of a friend of my own, who came by bequest into some cottage property. Being a man of honour, of intelligence, and of feeling, he proceeded at once to close the cellars, which had till then been used as separate dwellings. He went to some expense in closing the outer entrance, and in making a way into the cellars from the interior, so that they might serve as store places for the houses above. To his horror, however, he found that he had done nothing but mischief. The cellars were still underlet, and all that he had achieved was to make them even more unwholesome than before. To relieve such pressure as this, then, I calculated that about £100,000 would be needed. How was this sum to be provided? I thought it undesirable that the Corporation should become large holders of cottage property, and I thought, moreover, that they had already on their hands even more than they could well accomplish. So I determined that I must look to some other agency, that individuals or an association must find the money, and be content with such returns as they could get. Here was another difficulty. Was the enterprise to be recommended as a charitable or as a commercial speculation? Nothing excites more distrust than to go to men of business and mix the two inducements. They say, if you want a charitable grant, here is a cheque; if you want to engage me in an investment for profit, I may or may not join you, according to the prospects you can offer; But do not ask me in the name of charity or philanthropy to take shares in an insolvent company. As I have already said, the poor must be well housed if communities are to flourish,

whether it be at a direct gain or loss. Fortunately, however, we succeeded in drawing up a plan which seemed to offer such a return as to insure investors from loss, if not to tempt them with promise of gains. I thought I could secure the value of money at least, if not a trade profit, and that I might legitimately urge the preference of such an investment on philanthropic grounds. Having got thus far, I went formally before the principal bankers of Leeds, Messrs. Beckett & Co., Messrs. Brown & Co., and Mr. Turner, the Manager of the Bank of England. They all received me with the greatest courtesy and attention, and from them I have received up to this time the kindest support. After much discussion and the advice voluntarily given to us by Messrs. Tennent, Newstead, & Wilson, Solicitors, it was determined to form the Leeds Industrial Dwellings Company, Limited, with a capital of £50,000. This gave powers to expend a sum equal to one-half of that upon which I counted as necessary for the full relief of the town, and upon the security of this capital sum I proposed by sales, mortgage, or other means to continue operations up to a higher amount. The Loan Commissioners, again, are empowered to make loans at not more than four and a half per cent. on the security of such a company for the purposes of building houses for the poor. In order that working men might themselves take part in the movement, the shares were fixed at five pounds, with ten shillings paid on allotment, and ten shillings call, making a pound deposit; subsequent calls not to exceed one pound per annum. It was agreed not to rush at once into building on a large scale; first, because building was in 1866 at its very dearest point, and has since the completion of our present block come down at least ten per cent. in cost; secondly, because we wished to test by experiment the merits of any plan which we might adopt, and so profit by mistakes on a small scale instead of on a large one. It was determined, therefore, to erect an experimental block at a cost which, all expenses included, should not exceed £2,000, and this block, which is completed and inhabited, I now have the honour of describing to you.

At our first meeting instructions were given to form the company, and shares were at once taken up in sufficient numbers to meet all expenses. We have not as yet had to put any shares on the market. So far, incorporation expenses apart, we had been at no expense worth recording. A piece of ground was now, however, purchased at about five shillings a yard, which lies near a populous part of the

town, and is known as St. Ann's Square, Shannon Street. For this piece of ground Messrs. Adams & Kelly designed us a building to contain twenty-five tenements and a corner-shop. The block is four stories in height, and the first, second and topmost floors are traversed by galleries upon which the doors of the dwellings open. The galleries themselves are reached by an open stair at the end of the building. The dwellings on the ground floor, being brought up to the street line, are larger than those above, and support the galleries. The tenements are mostly of two rooms and three rooms alternately. One only consists of a single room. The shop at the corner consists of three rooms in addition to the shop.

Before I forget the single room tenement, let me say how strongly I feel with Professor Kerr the necessity for single-roomed dwellings in towns. Professor Kerr received far less attention than his proposal deserved, when he urged the building of such dwellings. There are many single men, single women, widows, and childless couples, to whom such dwellings are a great boon, and our single room was in great demand. Mr. Oddy, who manages our cottages for us, could have let a dozen of such rooms in one day. I did think of proposing a block of single rooms, but on second thoughts I see that no class of families should be housed apart. It is well to accommodate as many grades of society as possible in one block. The two-roomed houses do for small families, and the three-roomed houses for larger families. Each living room contains 1,410 cubic feet. There is in it a cooking range, a sink with constant water supply, a meat safe, a coal bin, and a cupboard. Air-bricks ventilate the meat safe, and make a communication between the rooms and the outer air. Being near the ceiling, they are not readily blocked up. There is convenience for a gas metre in each house.

At the end of the block, and reached by the main staircase, are waterclosets, dust shafts, and washhouses. We did not think it wise to be at the expense of a flat roof, as the smoke and dirt of such open places in a large town renders them almost useless. Nor did we put fire-places in the bed-rooms, as these are always stuffed up with bags of straw and the like. Warmth is received into the bed-rooms through the door of the living room.

I will now state what our expenditure has been, and what our returns are. Building was, as I said in 1866, at its most extravagant rates. At these rates our block has cost

£1,415 9*s.*; the land, £150; and the waterclosets, £50, making a total of £1,615 9*s.*, all inclusive.

Our revenue account is as follows:—

Accommodation.	£	s.	d.
One Shop at 4*s.* 6*d.* per week	11	14	0
One Single Room Dwelling, at 1*s.* 6*d.* per week ...	3	18	0
Five Dwellings, at 2*s.* 8*d.* per week	34	13	4
Five Dwellings, at 2*s.* 3*d.* per week	29	5	0
Three Dwellings, at 2*s.* 7*d.* per week	20	3	0
Three Dwellings, at 2*s.* 2*d.* per week	16	18	0
Three Dwellings, at 2*s.* 5*d.* per week	18	17	0
Three Dwellings, at 2*s.* per week	15	12	0
Gross Receipts ...	151	0	4
Charges	46	10	1
Net Income ...	£104	10	3

Probable Expenditure.	£	s.	d.
Shop.—Sewer, and Lamp Rate		18	0
Shop.—Poor Rates ...	1	16	0
„ Highway Rates...		13	6
Dwellings.—Poor Rate...	10	18	0
„ Main Sewer, etc.	7	9	10
„ Highway Rates	4	1	9
Water Rate	7	11	0
W. Cs., 2*s.* 6*d.* per house	3	0	0
To flushing do.	2	12	0
Collecting	7	10	0
	£46	10	1

Showing a gross return of nine and a quarter per cent. upon capital invested, and a *net* return of about six and a half per cent.*

This, Sir, I need not say, is a result most satisfactory to the shareholders and promoters. It will be seen that I have advised the directors to go to the expense of £50 in providing water-closets, and to an annual expenditure of £5. 12*s.* in supplying and working them. This is not the place for opening the discussion of water *versus* earth closets; I can only say I wonder that anyone who knows much of the working of large towns can have any doubt of the great superiority of water-closets.

The next doubtful question we had to decide was the washhouse question. There was much to be said against our providing them, that much being the cost of them. On the other hand, I need not say here how important an addition these are to the houses of working people. Strange to say, however, they are very little used, and are an unremunerative outlay. Even in Yorkshire, where coal is cheap, the expense of lighting fires out of the home is disliked.

* The remarkable collapse of trade following its unnatural inflation has made it very difficult to calculate the need for cottages. Of late we have had many tenements empty from time to time. A large cottage owner tells me that he has now (April 1869) forty empty houses to let.

To lighten this expense we put up boilers large enough for two families to use together, hoping thus to halve the expense of fuel. As yet the women have not learnt that essential mark of civilisation, viz., combination. I think it my duty nevertheless to urge that washhouses continue to form a part of our plans. When a healthier opinion begins to grow up among the artizans we shall be able to make a small charge for use of washhouses instead of finding them, as we now do, a dead loss. Ordinary builders, however, cannot be expected to make this sacrifice. There is a growing desire among working people to have healthier and airier dwellings, but they are not yet willing to pay a due proportion of their wages to secure it. I hope they will soon learn this true economy. Until quite recently few even of the most respectable artizans in Leeds dreamed of desiring more than one bed-room. They thoughtlessly allowed themselves to be satisfied with a state of accommodation which necessitated the most shameless disregard of decency. The utmost rent they would then pay was two shillings a-week. With the increased sense of propriety which the occupation of two-bedroomed cottages has given, the working man has of late years become willing to pay 3*s.* 6*d.* or 4*s.* in rent. The bedrooms are, however, very small in most cottages. It is a great question with me whether wholesome airy cottages, having two good bedrooms, or sometimes even three, should not be built by us, even if only to pay four and a half per cent.

The building societies are, however, doing much to help the best artizans to obtain good houses which they can be proud of as their own. In this sense of ownership a wholesome pride will grow up, the pride of owning a roomy, healthy house. All that we can do in building for the very poor, however, is to give a not unhealthy abode. We cannot go beyond bare necessaries. Our bedrooms contain about 980 cubic feet, and we admit families to the two- or three-roomed houses according to the number of members. No lodgers are admitted.

Working people are, however, very fond of collecting quantities of furniture, being unable to dissociate themselves from the desire for concrete and portable property. An ordinary artizan will often have furniture worth £30. This taste fortunately leads them to prefer large rooms, and induces a man and his wife to save. To acquire a big mahogany chest of drawers with bright brass handles is an object of legitimate ambition as being a visible evidence of wealth

and having a good effect upon neighbours. For well-paid artizans, therefore, rooms should be large and convenient. We are building, however, rather for persons now dwelling in cellars and garrets, and having no wardrobe to stow away.

Let me now enumerate the mistakes we have made. We have boarded the floors of all the living rooms in order to make them warmer. The result is that noise is propagated more rapidly through the building, and that our boards may soon be destroyed by dirty and careless tenants. A light concrete would be as warm and more durable. Our fire-places have their openings too wide: hence they tend to smoke and to waste heat. This we must remedy. Another error we have made is in giving insufficient coal accommodation. In saying this, however, I am, perhaps, speaking according to a Yorkshire standard. Our coal bins will not accommodate a load of coals, and to buy coal by the load is the most prudent marketing in Leeds, though it is unknown in London. The difficulty of carrying quantities of coal up to a fourth story is, however, very great, and the danger of a crane, if generally used, would be still greater. But the dwellers in the top story will be for the most part a people not troubled with great quantities of goods of any kind. The only other mistake I think of is that we have provided no cellar for our shop. In carefully avoiding cellars in the ground floor tenements, which would certainly be let as dwellings, we did not remember how important such a store place is to a shop.

No sooner, however, were our tenements to let than we had multitudes of applicants. The changes have been few since the first letting, and the rents have been well paid.* This is encouraging, as our present block was only built for, and is occupied by, the really poor—by the class in which the fever is bred. If we build again, however, we ought, perhaps, to arrange in a larger block, to meet the needs of a more varied population, and to avoid the isolation of the worse-paid labourer from the better. We must try not to separate the skilled artizan altogether from the unskilled, as such separations tend to weaken society, and to establish castes. A quadrangle containing houses of all classes, from 1*s.* 6*d.* to 5*s.* a-week, and including a club-room for the men, and, perhaps, a bath-house, is, I think, the best form, as it gives an open space in the centre for light and for a children's playground.

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* *Vide* note, p. 114.

It is, however, a matter of very great difficulty to secure land in the central parts of large towns, and when secured it is to be taken as we find it, often refusing to lend itself to ideal arrangements. Light and air, and solid construction, must be the essentials. As building is now cheaper than it has been, I should be glad to be able to announce to the Epidemiological Society the completion of a larger undertaking.

And now, Sir, I have little more to do than to thank you and the Society for the courteous forbearance with which you have listened to my long paper. I feel that I ought to have written it in less, but I may honestly say that I have tried to avoid going over old ground. I venture also to think, Sir, that I have your sympathy in the main, and that I have it so far as this, that if other towns can be persuaded of the deep importance of the question of housing their poor,—of protecting their housing as they ought to protect their food and their water,—and if they can be persuaded to form associations for securing a supply of dwellings, not "models," not fancifully adorned or expensively fitted, but simply wholesome,—that you will gladly forgive me for my lengthy call upon your patience.

ON THE MALIGNANT PURPLE FEVER EPIDEMIC IN IRELAND.

By E. D. MAPOTHER, M.D.,

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[*Read July 1st*, 1867.]

Mr. President and Gentlemen,—I have been asked to report to the Society some of the facts at present ascertained upon the nature and progress of the novel malady now in Ireland, and I therefore offer the following brief and hurriedly written notes, which I trust may serve to elicit the opinions of members present. My facts are derived from my own observations, from the cases reported to the Society of our College of Physicians, and from the Registrar-General's returns, to which my friend Dr. Burke has given me access.

The history and extent of the epidemic is as follows. A most healthy medical student, aged nineteen, fell ill with chilliness and *malaise* about noon on March 18th, 1866. His master, Mr. Croly, visiting him in the evening, found he had frequently vomited and was very prostrate; purple blotches appeared on his skin during the night, and about noon next day he suddenly fell into stupor, and was dead at two, or about twenty-six hours from the apparent commencement of the symptoms. Drs. Stokes and Benson, who also saw this appalling case, felt they had to do with a novel type of disease. There was nothing in personal history or residence to account for the origin of the case. A girl aged eighteen suffered from like symptoms on April 2nd, but recovered in the Hardwicke Hospital; and on May 12th, 13th, and 17th, fatal cases were noted. In August, while cholera was at its height, a lad died in Cook Street (the most intense focus of the disease) with similar symptoms. The disease was next heard of in January, 1867, cholera having ceased on December 28th. At Kingstown (six miles from Dublin) I saw a woman, aged twenty, who died of malignant purpuric fever in eighteen hours, on Jan. 16th. She was four months pregnant, and aborted two hours before death. The disease progressed, and up to the

present week 63 fatal cases have been registered in the Dublin district (including Kingstown, Donnybrook, and Rathmines), which is 9745 acres in area, and contains 314,409 persons. Besides these deaths, about eight soldiers have died of it. The other districts attacked, and the approximate number of deaths, are, Tullamore, 3; Parsonstown, 5; Mitchelstown, 1; Rathcoole, 1; and Clondalkin, 3. As medical officer of health in Dublin, I investigated the circumstances of 41 fatal cases of the positive type, and they were distributed over the twenty-two weeks from Jan. 19th to June 15th as follows—1, 1, 1, 3, 0, 4, 1, 3, 1, 0, 1, 1, 3, 4, 5, 1, 0, 4, 1, 1, 1, and 4. I will presently adduce some other statistical facts concerning these cases. Out of Ireland, Chelsea is the only place in which the malady has proved fatal. Dr. Crisp recorded a death on the 17th of June by this identical disease.

The *synonyms* which have been applied are as follows:—"Black death"—an appalling and most inappropriate term, as it bears no resemblance to the bubo plague of the fourteenth and seventeenth centuries,—"black fever," "cerebro-spinal meningitis," "*meningite foudroyante*," and "malignant purpuric fever." To me, the word "virulent" seems preferable, as it pointedly indicates the blood-poisoning; and the word "purple" is similar to, though less technical than "purpuric." Many deaths in the London bills of mortality were noted as "spotted fever and purples" down to the seventeenth century. "Petechial or spotted fever," the term employed by American writers, is not distinct enough from ordinary typhus. As "pernicious fever" the disease was perhaps identified by the older French and Italian physicians; and the "febris scorbutica" of Sennertus (1654) was a similar if not an identical malady. If it shall be determined that it is a lesion of the nervous system, ganglionic or cerebro-spinal, the term "neuro-purpuric fever" will be descriptive. It is most needful that some one name shall be fixed on and followed, for the present redundancy greatly confuses statistical inquiries.

A description of the symptoms must include two types, which are diverse enough to create in the minds of many acute observers a doubt of their unity. In the graver, life is rapidly extinguished as if by a blood-poison; in the milder, the symptoms are those of inflammation of the cerebro-spinal axis or its membranes. Dr. Stokes regards these latter phenomena as secondary to the essential disease, and they appear if the patient lives long enough for their develop-

ment. Bearing in mind that the nervous system controls the motion of the blood and the process of nutrition dependent on it, as well as locomotion and sensation, I think all the symptoms may be referred to lesion of some part or other of the nervous system; and I would propose to divide them into vaso-motory, and locomotory and sensory—the two latter being grouped, for simplicity, as cerebro-spinal. Chilliness, or sometimes a severe rigor, depression of spirits, and the conviction of some serious illness impending, are the earliest symptoms; and vomiting (probably due to stimulation of the pneumogastric nerve) follows, and is often frequent, persistent, and distressing. The contents of the stomach and biliary matter constitute the ejecta. Constipation is usual, and remains till near death, when involuntary discharge occurs. In one case I observed melæna. Dryness is the only condition which characterises the tongue. The pulse is always more compressible than in health, and usually beats over 100 per minute; it never assumes the force or hardness indicative of acute local inflammation. The dark purple blotches of extravasated blood (or rather dissolved hæmatin, for there are not capillaries ruptured to let out the cells) are situated in the true skin and areolar tissue under it; and the legs, hands, face, back, and neck, are, in the order enumerated, the places in which they usually appear. They are frequently raised enough to be distinguishable by touch, and vary in size from that of a pin's head to that of a walnut. I have not been able to bring them out by pressure on the skin, as may be done in purpura, the blood being so thin. Bullæ of red serum have been seen in a few cases. The skin generally possesses a dusky hue, and is usually moist—even bedewed with sweat which the palsied capillaries may pour out. No accurate thermometric observations have been made, except in two cases by Dr. Stokes. In one the temperature was on the third day 96.7 deg. and never reached 99 deg.; the pulse was only 80. In the other case the temperature was 99·6 deg., and the pulse 84, on the second day.

It may be well to relate one case briefly, and I select the following, as it is the most recent, and the patient was of greater age than any other victim:—

Mrs. Kate D——, a healthy woman, aged fifty nine, the cook of a large establishment, which in every respect was wholesome, complained on the night of the 25th of June of chilliness. The manager compelled her to go to bed, and take some brandy. On the 26th she rose apparently well,

and attended to the cooking till 6 p.m., when, feeling weak, and having vomited, she went to bed. Vomiting was the only symptom noticed during night by the other servants in the dormitory with her. At 9 a.m. on the 27th, when I saw her, dark purple spots had appeared on the back, chest, and legs. Her pulse was 110, and very compressible; respiration very embarrassed, and but 11 per minute. There was considerable stupor; she would every few seconds turn from one side to the other, but did not speak. When asked in a loud voice if she was in pain, she muttered, "No." I had her removed to the Hardwicke Hospital, where she died at 2 p.m., twenty hours after her serious illness began, or forty-two from the occurrence of a chilliness. On the 29th of June —for reasons I shall presently mention—I inquired of her daughter if there was any peculiarity about her food; and she at once answered that for as long as she remembered she would never touch vegetables, and that she always had the best of everything. She complained of having to sit up to very late hours, and as the other servants rose early her rest in the mornings was broken.

In such rapidly fatal cases post-mortem examinations have shown congestion of the pia mater; some opacity of the arachnoid; the effusion of reddish serum; fluid blood gorging the lungs and right side of the heart. In one in which the patient lived three days there was not a trace of inflammatory action anywhere, and nothing to be noted but passive congestion of all the larger veins. Decomposition progresses with amazing rapidity; and in one case the odour from the body within a quarter of an hour after death was horrible.

I have seen no positive case of epidemic cerebro-spinal meningitis such as those in Dublin (1846) or Dantzic (1865), in which there were no purple spots; and, therefore, I condense the following from the report of Surgeon Haverty, 52nd Regiment. Private H——, after being on guard all night, complained on the 24th of February of severe headache; and at 4 a.m. next morning was tossing about and almost shouting with pain. Pulse firm and resisting; tongue dry and brown; skin hot and harsh. Leeching, calomel, and James's powder, and enemata, produced relief; but at 1 a.m. on the 26th the above symptoms returned, and continued with intermissions till March 1st, when severe pain and tenderness along the whole spine occurred. He remained in this state till the 11th, when delirium, intense restlessness, and laboured breathing were succeeded by death—fifteen days after admission to hospital. There was

found intense meningeal congestion, opaque patches in the arachnoid, copious deposition of lymph, and traces of purulent matter about the pons and medulla oblongata.

Sudden seizure of pain in the head and back, so severe as to make the patients scream, great sensitiveness of these regions, and retraction of the neck, have been usual phenomena, and, in a few cases, opisthotonos greater than in tetanus has been noted. Yet in some such cases recovery has followed. In most of these cases there was pyrexia. Palsy of the face or limbs, or convulsions, occur later, and death is by coma.

During recovery nutritional changes in that field of visible pathology, the eye, such as corneal sloughing, iritis, and hypopyon, have been observed as in the Prussian outbreak. Such are due to lesions of the fifth nerve. The right eye has been alone affected. The effused blood-materials under the skin do not become absorbed, but frequently fall into gangrene. As might be anticipated, intense vascularity of the pia mater, opacity of the arachnoid, and effusion of lymph or semi-purulent matter are found along both brain and cord. In one case the entire spinal pia mater was smeared with pus. Such a type of disease closely resembles ordinary cerebro-spinal arachnitis, or that developed in the course of typhus or scarlatina—a combination which has of late been much observed in Dublin.

Herpetic eruptions pretty often came out during convalescence in this form of the disease, and they were probably the peripheral effects of central nervous lesion, more than attempts at cutaneous elimination.

In the first type I have sketched, that there is greater engorgement of the vaso-motor than of the sensori-motor system is evident from one of Dr. Lyons' cases, in which the boy was pulseless, yet sat up in bed, and anxiously inquired "What are these spots?" From this and other cases, he regards the disease as showing "an absence of specific lesion of the nervous system."

Dr. Gordon says—"The cases of cerebro-spinal meningitis which have been seen during the present epidemic may be subdivided as follows:—1st. The cases which were characterised by the suddenness and gravity of the symptoms; usually more or less lesions of innervation and extensive dark-coloured eruption, accompanied by copious subarachnoid effusion, cerebral and spiral, and of various kinds. 2nd. Cases almost or altogether as grave, indicated by very much the same set of symptoms, in which scarce any cerebral or

spinal effusion was found, but always great congestion. 3rd. Cases analogous to those of cerebro-spinal meningitis sporadically met with, where the symptoms of spinal irritation were well marked, and where lymph or pus, or both, were more or less copiously effused."

The most rapidly fatal case was that of a healthy schoolboy, aged ten and a-half years, who sank in less than five hours from the time of his seizure; and Dr. Gordon, who, in his admirable essay *(Dublin Quarterly Journal* of May, 1867), fully details the case, ascribes death to pressure on the respiratory nerve centre, and the circulation through the brain of highly carbonised blood. As in many other diseases which we describe as blood-poisoning—scarlatina, typhus, or cholera—the extinction of life may be most rapid. Whether this depends on the largeness of the dose or the peculiar susceptibility of the patient we know not.

The average duration in 41 investigated cases, which include both the blood-poisoning and cerebro-spinal types, has been forty-two hours, 14 of them having terminated within twenty-four hours. There being no registration of disease, the average mortality can be only approximated. Up to May 1st, Dr. Gordon states, it was 80 per cent.; but, like other epidemics, it has decreased in virulence, and about half those now seized die. Age is the strongest predisposing circumstance, as will be seen from the following distribution of 41 cases according to quinquennial periods from five to sixty-five—6, 6, 6, 8, 5, 3, 2, 1, 2, 0, 1, 1. If the disease be essentially neural, the dominant action of the sympathetic may account for the proneness of children to take it; or if it be a blood-poison, the removal of temporary organs at that period would afford abundant fibrin, the pabulum for zymosis. It is certainly that material which responds to the poisons of the communicable diseases which are most readily absorbed in childhood, when nutritional changes are so active. Of these fatal cases, 21 were females, and 20 males. As to rank in life, a young nobleman, three medical students, two undergraduates, several persons in well-aired and well-drained houses, and some from wretched tenements where typhus always abides, and where cholera haunted last year, have been victims. It may be noted that all the towns which have suffered from purple fever were also visited by the partial epidemic of cholera last year, which suggests some relation in the promoting causes of both maladies. There are only two instances in which, from the occurrence of more than one case, a probability of contagiousness was suggested.

1st. That of a wretched hovel from which cholera had snatched victims last year, and in which a child, its sister, and lastly its mother, were attacked with purple fever; the latter two being seized simultaneously ten days after the first patient died. 2nd. That of a cabin built in a filthy swamp, and so unwholesome that the mother had lost her six children by zymotic disease; here a second child sickened forty-eight hours after the seizure of the first. In both of these dwellings sanitary circumstances were so bad that the inmates must have been to the utmost prepared for any zymotic. It is possible there may have been some hereditary proneness — a circumstance so constant with regard to hæmorrhagic diathesis.

If purple fever be due to the same morbid poison as typhus (a view I shall just now support), its lesser contagiousness is sufficiently accounted for by its more rapid fatality, for it is at the period of elimination that typhus and other specific fevers are most communicable. Excitement and fatigue must be ranked as predisposing causes. The Flying Column of the 2nd Regiment had been pursuing the Fenian rebels when the disease arose, but ordinary typhus was prevailing at the time in the regiment. Nearly all the victims in the French outbreak were young conscripts just torn from their friends to undertake harassing duties; and in our constabulary depôt, in 1846, an outbreak occurred amongst the recruits. The mental disquietude during the American war may have promoted the outbreak in that country; and some of the adults attacked in Dublin lately were persons whose natural sleep was much interrupted.

The meteorology of the first half of this year has been remarkable for the unexampled coldness of March and April, and the constant prevalence of east wind during those months and May.

The deaths by concurrent epidemics during the past six months are compared with those of the corresponding periods in 1866 and 1865 in the following table:—

	Fever.			Measles.			Scarlatina.			Diarrhœa.		
	1865.	1866	1867.	1865.	1866.	1867.	1865	1866.	1867.	1865.	1866.	1867.
January	49	46	39	3	11	5	4	3	7	13	12	19
February	42	50	30	0	7	10	6	7	8	10	14	16
March	47	40	32	7	4	25	3	2	5	9	16	21
April	37	44	24	7	2	66	2	2	6	12	17	27
May	40	56	26	14	1	56	2	6	9	7	5	10
June	42	35	29	23	1	58	2	4	17	9	14	23

Fever, then, has been notably less; but, perhaps, we are

not justified in subtracting these novel cases from that category. Measles has been generally diffused and very fatal, a few of the deaths being registered as "rubeola nigra." The concurrence has been noted in other cerebro-spinal epidemics. In one of Dr. Stokes's cases measles came out during recovery from the malignant purple fever.

For the past eighteen months "purples" has been very rife among pigs, many carcases having been seized by the clerks of the markets. I think there is much resemblance between this disease and human purple fever, and Mr. Hugh Ferguson, the Veterinary Officer of the Privy Council (for such we have in Ireland, though no Public Health Department) has favoured me with the following particulars of the disease:—"Great and rapid mortality, many of the animals dying within half an hour after they had appeared to be well and to feed voraciously; others are indisposed for two or three days, and die within an equal time from their being seriously ill. In all, purple blotches are found, especially in the thinnest and least hairy parts of the skin. Nervous symptoms vary: in some cases marked excitement; in others, lethargic depression and muscular atony. Tetanic tendency is characteristic of the purest type. The *post mortem* examination presents general congestion, with a tarry, uncoagulated blood, and numerous ecchymoses under the skin, over the membranes of the brain and medulla, and into the substance of the muscles, even that of the heart." He states that "purples" among pigs was very prevalent during 1846, when cerebro-spinal meningitis was epidemic at Dublin and Belfast. Several oxen have died very suddenly during the past two months; effused blood on the brain and along the mucous surfaces has been discovered on dissection. Mr. Ferguson regards the disease as congestive apoplexy, induced by overfeeding in rich grass. Splenic apoplexy of the ox and sheep, which is due, according to the French veterinarians, to the entrance of the parasite bacteria from the air into the blood, presents phenomena by no means dissimilar from the "purple fever." While in Paris lately I obtained specimens of the bacteria in ox blood. Healthy animals infected with such blood get the splenic apoplexy, and it is said that their flesh, if eaten by pigs, gives rise to meningitis. The resemblance between such diseases in the lower animals to this in man, and the possible dependence of the human malady on food derived from diseased animals or a diet scanty in vegetables, are sufficient to illustrate the need which exists for the scientific study of epizootics. In Ireland

we have no means whatever for instruction in comparative pathology.

I will next offer you a brief analysis of the facts of previous epidemics, related by the best authors.

From 1838 to 1840 the garrisons of Versailles, Nancy, Lyons, Strasbourg, and some other towns were attacked with cerebro-spinal meningitis, as it was at once called; for dissection displayed lymph and semi-purulent matter abundantly under the cerebral and spinal arachnoid. The symptoms were of a correspondingly inflammatory type, as detailed in Rollet's *Memoir* (Royal Academy of Medicine, 1843). Cutaneous extravasations or eruptions did not appear at Nancy, while they did at Versailles. In the former garrison the mortality was 28, in the latter 42 per cent. The patients were almost entirely conscripts under twenty-seven years of age, in whom long marches, the anxiety of new duties, and regret at parting from relatives, were supposed to be exciting causes. It was believed to be non-infectious.

During the months of January, February, and March, 1846, several cases of cerebro-spinal arachnitis occurred in the South Dublin and Bray Workhouses, and in the Belfast Workhouse shortly after. Dr. Mayne described the disease in the *Dublin Journal*. Boys under twelve years of age were the sole victims, girls under circumstances quite similar escaping. The arachnoid of the cord especially was found opaque, and lymph most freely poured out under it. Hot skin, full pulse, from 120 to 140, indicated acute pyrexia. The limbs, neck, and face were tetanised, convulsions or coma preceding death, which occurred usually on the fourth day; but one case ended in fifteen hours. Increased sensibility of the skin was common, and exquisite soreness of the epigastrium constant. No cause whatever could be assigned, and treatment proved useless.

In the United States, from 1811, when Dr. North well described the disease, it has been frequently epidemic, and attracted much attention in 1863. The American journals contain many essays, which are ably summarised in the last edition of Wood's *Medicine*. Two forms are recognised,—one marked by shock, weak pulse, purpled limbs, and coma, death happening within the first day; the other presenting signs of cerebro-spinal mischief, such as tetanoid spasms and hyperæsthesia, death occurring in three days. Purple spots were present in twenty-seven out of forty-four cases. Congestion in the former, and effusion of lymph in the latter, was the cerebro-spinal lesion found after death.

The outbreak in Dantzic, in 1865, is ably recorded in your *Transactions* by Professor Hirsch, and by Dr. Sanderson in the Privy Council Report. Of the 2000 cases in West Prussia, half died; and of 347 cases, 330 were under fourteen years of age. No mention is made of purple spots, and the lesions and phenomena were wholly cerebro-spinal. It was believed to be non-contagious. Cold weather and the crowding of the poor were the only exciting causes suggested.

I will conclude with a few suggestions upon the nature of the disease. The rapid form, with purple blotches, and without cerebro-spinal symptoms, is certainly a zymotic or blood-poisoned malady; but that the other type, characterised by positive signs of cerebro-spinal lesion, without the blotches, is due to the same proximate cause I am not convinced. On the other hand, no inflammatory lesion of any organ whatever could be so acute as to kill in five hours. The letting loose of the blood into the skin, where it remains till it decomposes; the weakened arterial force; the embarrassed respiration; and the nutritional changes in the eye, which make up the phenomena in the rapid form, may be no doubt explained by supposing that a blood-poison depresses or arrests the contractile power of the vaso-motor nerves. There is no place where these abound more than in the membrane which after death is found mainly affected. The supply of sympathetic filaments to the arachnoid is most abundant; in fact, in or under that membrane they form a mesh before being distributed to the cranio-spinal nerves and bloodvessels. The greater proclivity of children to cerebro-spinal meningitis may be due to the greater proportional activity of the sympathetic system. In his last weekly report Dr. Farr has given the leaven of an important question, namely, "Is this purple fever only typhus seizing the scorbutic?" The best of antiscorbutics, the potato, has increased in price, as also have green vegetables; so that the poor use less. In 1846 the potato was here unattainable by them, and we had cerebro-spinal arachnitis; and about the same time, as our medical registrar, Dr. Burke, reminds me, purpura and scurvy were most frequent. During the past eighteen months I have seen many cases of purpura, and in England the deaths by it were one-third more numerous in 1865 than in 1854. Spring and early summer have been always the prevailing periods for the epidemic meningitis or purple fever, and then the potato is scarce and bad. Our poor mainly subsist on bread and tea and Indian meal, a diet

highly promotive of scurvy; and children who suffer most from the epidemic are especially apt to want fresh vegetables amongst their food. Purple fever has doubtless stricken down the well-fed, but it is just possible that, like the cook whose case I detailed, they may have had a habit of abstaining from vegetable food. Scurvy is of all maladies the most asthenic, and one may well believe that a sufferer from it receiving the typhus poison would sink at once, no time being afforded for elimination of the poison, as we see occasionally in virulent cases of scarlatina. Dr. Murchison has contended that epidemic cerebro-spinal meningitis is but a form of typhus. If this be so, it is not surprising that with us it should be attended more frequently by cutaneous blotches, seeing that petechiæ are much more frequent in the typhus of Ireland than elsewhere. Stokes ascertained that they appeared in 386 cases out of 540 in Dublin, and if typhoid cases had been distinguished (as they nowadays should be in the registers), the proportion would be much greater. The faulty diet of our poor may account for the greater frequency of this blood condition.

Of treatment I will only say that if there be vaso-motor depression, ergot in scruple doses, belladonna in grain doses, every second hour, and perhaps cantharides, are worth a trial, as they are contrary in physiological action. Alcohol and digestible nutrients, such as solution of beef in dilute hydrochloric acid, might give aid; and the hot air or cotton bath might tend towards cutaneous elimination. With regard to prevention, although I saw no reason to believe that the disease was communicable, I have directed disinfection of rooms and clothes, and isolation of the patient as much as possible. If there be truth in the dietetic point I have touched on, and which I will further investigate, it becomes one of the most preventable diseases which we encounter.

November, 1868.—The epidemic has almost died out—one or two cases having occurred about each month since; no etiological information has transpired. Mercury has been in my hands very useful in the inflammatory and somewhat prolonged cases. One case terminated fatally by effusion of blood into both lateral ventricles of the brain.

ON THE

SO-CALLED EPIDEMIC OF "MALIGNANT PURPURIC FEVER IN IRELAND,

AS IT OCCURRED AMONGST THE TROOPS.

By Dr. JEFFERY A. MARSTON,

STAFF SURGEON.

From Records in the Army Medical Department.

Read 1st July, 1867.

In the present paper, the army medical experience of the disease which has been variously designated Pestis, Malignant Typhus, Cerebro-spinal Typhus, Malignant Purpuric Fever, and so on, is passed in review.

For the sake of clearness, it has been deemed better to arrange the facts under two divisions. The first includes a short medical history of the occurrences, arranged in their chronological order. The second takes note, from a clinical and pathological point of view, of the morbid phenomena presented by the cases respectively.

Having various sources of information at command, it has seemed to us better to describe the progress of the disease from our own point of view, and we have included all those medical and collateral occurrences which appeared to reflect any light on the subject, or to be referable to similar if not identical causes. The disease has, so far, been confined to Ireland.

The first case, recorded by a military surgeon, occurred in the 52nd Regiment of Light Infantry, stationed at Richmond Barracks, Dublin. Very shortly, it was this: Private H—— was on guard at the Magazine Fort on Feb. 24th, and came to hospital next morning. He exhibited lividity of countenance and symptoms of extreme pulmonary distress, for which no cause intrinsic to the thoracic viscera could be discovered. During his illness he suffered from symptoms of cerebral oppression, vomiting, purging, hiccough, and great pain in the back and head, succeeded by coma. He died on March 11th, 1867, fifteen days after admission into hospital. No spots are spoken of. The post-mortem examination showed great

congestion of the membranes of the brain, and opacity of arachnoid in patches; the congestion being most marked at the base of the brain, and accompanied by lymph and traces of purulent matter about the pons and upper part of medulla oblongata.

Surgeon-major Cogan, the medical officer in charge of the 2nd battalion of the 2nd Regiment at Birr, Ireland, had a child under his care attacked on the 8th of March. The child had apparently been undergoing for some months a gradual deterioration of health, but seemed rather better on the day of attack. The symptoms were: vomiting, fever, pain in the head, back, etc.; purple, livid spots on face, back, thighs, legs, and feet; and indications of a cerebro-spinal irritation. On the 14th, the eruption of measles appeared. This child ultimately recovered after a very long and severe illness. The details of this case are extracted from a report by Dr. Cogan.

A reference to the medical returns of the 2nd battalion of the 2nd Regiment for the week ending the 29th of March, shows one death from apoplexy. Attention has been directed to this. It appears that a private, of the age of twenty-one, had been in the regimental hospital since the 17th of March for primary syphilis. On the 27th apoplexy supervened; death in thirty hours.

An abstract of the post-mortem appearances (nineteen hours after death) mentions: "Body rigid, with livid spots about the neck and trunk. Membranes of the brain congested; substance of the brain firm, but much congested; a small quantity of straw-coloured fluid in ventricles, and a trace of pus was found in the posterior part of each lateral ventricle." No pathological changes in other organs, sufficient to account for death, were discovered.

The 85th Regiment was stationed in the Royal Barracks, Dublin. The patient was a young soldier. He was admitted into hospital March 19th, under the head "encephalitis," and died of coma after three days' illness. No further history or details of post-mortem appearances received.

The depôt of the 83rd Regiment embarked from Dublin on the 27th of March to join the depôt battalion at Colchester. On the 22nd a death took place at Dublin. Private W—— was sent to the General Hospital on that day "in a state of partial collapse," and he died on the 23rd, under the head of "typhus fever," after an illness of from forty-eight to sixty hours.

The *post mortem* appearances are thus described:—Brain

and lungs deeply congested; cavities of the heart filled with dark fluid blood; small intestines congested throughout, and presenting livid spots in many places.

The 83rd Regiment embarked from Dublin on the 6th of April, and arrived at Gibraltar on the 13th. No medical occurrences of any importance took place during the voyage, with this one exception—viz., on the 9th of April a woman is reported to have died of "purpura" after twenty-four hours' illness.

So far we have described cases, some of which have, and some of which have not, a distinct reference to the disease in question. Their relevancy will, however, be rendered manifest when read by the light of the subsequent events in the 2nd Battalion of the 2nd Regiment; and particularly in reference to the prominent position assigned by the medical officer of that regiment to the influence of the diffusion of the typhus poison by the return of the men who formed part of the "Flying Column."

A private soldier of 2nd Battalion, 2nd Regiment, aged twenty-nine, died on 31st March, in the Thurles Union Fever Hospital, of what was supposed to be a malignant form of typhus fever. Two cases of the disease were admitted; one, as we have said, of a malignant type, which rapidly proved fatal (twenty-seven hours' illness); the other recovered. We have no skilled or educated observation on which to rely; but the orderly, who subsequently attended upon an undoubted example of the disease at Birr, remarked that one of the typhus patients at Thurles was covered with the same livid purple spots when he died that he (the orderly) had seen on the patient at Birr.

The medical officer in charge of the "Flying Column" was unable to account for these occurrences. The whole district was, he believed, exempt from fever during March, and the men attacked had, as it happened, undergone less exposure than their comrades; moreover, these two men of the detachment of the 2nd Regiment were said to have been the first occupants of the fever ward of the civil hospital.

The "Flying Column" was now broken up, and the detachment marched from Thurles to Roscrea. On their arrival at Roscrea (5th April) a soldier was taken ill and sent to the regimental hospital at Birr, where he was found to be suffering from the symptoms of typhus fever.

We now come to the commencement of the outbreak of the disease in the regiment.

On the 13th April, at two A.M., a child was attacked with

fever, vomiting, and purging, succeeded by dark-purple spots on the body and extremities, unconsciousness, coma, and death in seventeen hours. The father of this child had not been out with the Flying Column, but the mother washed for the men who had formed part of that column.

The two next cases occurred on the same day—that is, on the morning of 14th of April.

A young married woman, the wife of a sergeant who had returned from the Flying Column, was seized with symptoms of the disease—fever, cerebral disturbance (incoherence), the appearance of large dark-purple spots on the skin—succeeded on the third day by great pain in the head, and serious disease of the left eye, with purulent deposit in the cornea.

A recruit was attacked in a somewhat similar manner. His skin presented some of the characteristic spots; he laboured under fever, headache, delirium, followed by the signs of cerebral oppression amounting to loss of consciousness. Obstinate constipation was a marked and exceptional symptom in this case, which ended in recovery. The mother of the above-named child washed for this man, and he slept in the same room with men who had been out with the Flying Column.

Dr. Ross, 92nd Highlanders, proceeded to Birr, with instructions from the major-general commanding the division to confer with the medical officer of the 2nd Battalion, 2nd Regiment, and furnish a report. This report is dated Birr, April 16th. It throws but little light on the subject. Two cases of typhus fever were in the hospital—one severe, the other convalescing. Every precaution against contagion had been adopted. The position and sanitary condition of the hospital and barracks are reported as excellent in every way; the town of Birr was clean and healthy, and no disease existed among the civil population. Dr. Ross considered the disease to be a malignant form of typhus, and he, accordingly, deemed it highly contagious.

The next case reported occurred in the 52nd Regiment at Richmond Barracks, Dublin.

A young soldier was admitted on the 16th of April with symptoms of fever. He first exhibited dangerous indications at two A.M. the following morning. The symptoms were those of spinal irritation and cerebral oppression. A few petechial spots appeared on the thighs. Death ensued at half-past eight A.M., twenty-three hours after his admission. Membranous lymph about the pons and upper part of medulla oblongata, and great congestion of membranes of the base of the brain, are mentioned in the abstract of *post mortem*

fingers and finger-nails livid and numb. No headache or symptoms referable to the cerebro-spinal centres in this case. The prostration rapidly increased, and he died at half-past twelve P.M., sixteen hours after admission into hospital. The body, after death, was covered with purpuric blotches, particularly about the thighs and legs. Besides an unusual quantity of serous fluid at the base of the brain and in the spinal canal, nothing abnormal was detected in these parts. The heart was stained with mulberry-coloured spots over its surface.

The next case occurred, on May 1st, in the 85th Regiment, stationed at the Royal Barracks, Dublin. The patient was a young soldier aged nineteen years.

On the 6th of May, another case of this disease occurred in the 2nd Battalion, 2nd Regiment, at Birr.

On May 6th, a case occurred in the 52nd Regiment, while still occupying the Richmond Barracks. The patient was a young man, nineteen years of age. The attack is said to have commenced with a rigor on the morning of the 6th. He was sent to hospital, and merely complained of slight headache and pain in the limbs during the day. About half-past six P.M., however, symptoms of a serious nature rather suddenly set in—prostration and drowsiness, with an altered dusky hue of the countenance. A few livid spots appeared on the inferior extremities; vomiting, pain in the head, irritability of the bladder, and muscular spasms were evidences that the cerebro-spinal centres were implicated in the diseased action. This man recovered, being reported convalescent on the 24th of May.

We now come to the 48th Regiment, which occupied the same barracks as the last-named corps—namely, the Richmond Barracks.

A man was admitted into hospital on the morning of the 11th of May, complaining of pain in the head and shivering. Towards the middle of the day livid spots began to appear, and continued to increase in number and depth of colour until his death at twelve the same night. He was comatose for ten hours, and death ensued in fourteen hours from the time of attack. The autopsy revealed congestion of the abdominal organs generally, a similar state of the brain, some effusion of blood into the sac of the arachnoid (?), a deposit of lymph on the surface of the brain, and about a drachm of fluid in the ventricles.

On the evening of the same day (11th) another man of the 48th was admitted into hospital suffering from symptoms very closely resembling those just described. No petechial spots made their appearance, however, and he recovered.

his chest, abdomen, arms and legs covered with dark-purple spots. He died at half-past three P.M. Duration of illness about twelve or fourteen hours. Retraction of the head was present in this case.

The *post mortem* examination showed great congestion of membranes of the brain, particularly on the under surface, where there was a small quantity of effused lymph. The substance of the brain was thickly studded with red spots of irregular shape and size. The ventricles contained a quantity of fluid, and the arachnoid covering of the spinal cord showed a network of bright-red vessels.

Corporal Wm. T——, 52nd Regt., age thirty-nine; service, twenty years; admitted half-past four A.M. on June 11th, with prostration, nearly to loss of pulse, sighing respiration, inability to speak or answer questions, hot skin, and an eruption of dark-purple spots over body, arms, and legs. He died at half-past six P.M. Duration of illness fourteen hours.

The upper surface of the brain was covered with effused lymph, thicker at some parts than at others; the bloodvessels were congested with thick dark blood. On the inferior surface of the brain the membranes were congested, but no lymph had been effused. The arachnoid covering of upper part of spinal cord presented a network of congested vessels. The substance of the brain was thickly studded with bright-red spots. The ventricles contained some fluid, and the choroid plexus was dark-coloured.

During life the urine was examined; it was of natural colour, sp. gr. 1010, acid reaction, and did not contain albumen. This case was treated in the same hospital and by the same medical officer—Assist.-surgeon Whittaker, 69th Regt. —as that of Private Thos. H——.

The questions in which the main interest is centred are those concerning the nature, origin, and spread of this disease. We purposely avoid discussing its nature, or its pathological and etiological affinities to other diseases. As to its origin, all we know is, that it is no new disease, that it has prevailed in a sporadic way in Ireland for some years, and epidemically in other countries. When we regard the frequency and prevalence of diseases of the epidemic type of late years, we are insensibly tempted to refer to the operation of some such widely diffused influence as is implied in the expression "an epidemic constitution of the season"—an expression which recognises, but in no way accounts for, the facts. Still, epizootic and human diseases have been very frequent of late years. What their influence may have been, and what may

blance and relation between them for us to group them under some common name, and identify them as acknowledging an allied, if not identical, causation and pathology.

For practical purposes these varieties might be thus classified:

1. The worst and most speedily fatal cases presented symptoms, to our minds, expressive of a profound blood-poisoning; —the imperfect collapse, followed by a sopor and insensibility which speedily ran into coma; the paralysis of the pulmonary function, and the rapid appearance, as well as the number and extent, of the spots. These things left no room for doubt, in our opinion, as to the formidable nature of the malady, and the inadequacy of a cerebro-spinal meningitis to explain all its features.

2. As to the cases in which the symptoms of cerebro-spinal inflammation were the most prominent. Delirium and other manifestations of a disordered intellect were neither so common nor of such grave import as those symptoms referable to the spinal system—the convulsion, the retraction of the head, the spastic contraction of muscles, etc.; and yet some of the best marked of these cases recovered.

It is to be remarked, also, that there was no necessary relation between the occurrence, the number, or the extent of the spots upon the skin, and the amount of intra-cranial and intra-spinal mischief.

Diseases of this class, of course, require time for the development of the specific lesions which characterise them. In those cases which were very rapidly fatal, the morbid anatomist could not expect to find the same traces of diseased or inflammatory action as in those less speedily fatal; yet we have seen that a patient may perish after fourteen hours' illness, and indubitable marks of inflammatory action may be present in the meninges of the brain and spinal cord. In none of the fatal cases examined were these parts normal; there was excess of serous fluid, vascular congestion, or actual extravasation of blood present, and a careful analysis of the cases leads to the inference that it is to the presence of those fluids about the respiratory nerve-centres that the imperfect collapse and pulmonary distress are due. Whether the cerebro-spinal affection—says Dr. Haverty, the surgeon of the 52nd Regiment—be taken as the essential expression of the disease, or be looked upon in the light of a complication only, it is that which evidently gives it most character all through, and which drags its slow process of repair over a period of many weeks, perhaps months, attended with little or no functional derangement of any kind, but with extreme

NOTES OF EPIDEMICS IN SOUTH AFRICA IN 1866.

By INSPECTOR-GENERAL ROBERT LAWSON.

[*Read November 4th*, 1867.]

During 1866 no epidemic of a very serious nature occurred in the command, though there have been several minor manifestations of a pervading influence. There had been a good deal of fever of the typhoid form at Cape Town in the early part of 1865, attributable to an obvious local cause, but none of a serious description, and no death from it elsewhere. One case proved fatal, nominally intermittent, in the 67th at King William Town; the patient came from China the previous year, and was in an unhealthy state. In 1866, without any increase in the prevalence of fever, the cases among the troops were more severe, and deaths occurred from its continued forms at all the chief military stations. Among the civil population, both remittent and continued fever were very common in Maritzburg in Natal, from February to April inclusive. In the Orange Free States, where the Boers had been at war with the Basutos, and the latter were badly off for provisions, fever became very prevalent among them in April, and some time in May the Boers were also suffering much from it. Fever became frequent in Cape Town in August, and, as the summer came on, it increased; and during January and February of the present year (1867), there was a great deal of it, accompanied by unusual scarcity of water. At Port Elizabeth, also, fever was unusually prevalent in February and March 1867. There were a considerable number of cases of severe common cholera at Cape Town in February 1865. There was one case at Port Elizabeth on October 31st the same year, and another incipient case in a man of the 96th Regiment, who had embarked in the *Renown* at the same place on November 9th, and was attacked three days afterwards, in latitude 33 deg., 13 S., longitude 27 deg., 6 E. Early in March 1866, another case occurred in a man of the Engineers, on his passage from Mauritius to this colony in the *Alumbagh*, about midway between that island and Algoa Bay; she sailed on February

of the Cape Colony. Towards the end of December, it had pretty nearly disappeared in Natal, but still lingered around Graham's Town. In the early part of May, rust became very general in the growing crops of oats, and injured them most seriously; it was repeated again, even to a greater extent, in those sown at a later period on the approach of the warm weather. In May, the potatoes in the district of Uitenhage, near Port Elizabeth, were much injured by the disease.

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and another under the right armpit, both of which were suppurating; a sloughing carbuncle of the size of a crown-piece upon her back; eyes bloodshot; tongue furred; pulse feeble. She had been ill eight days, and the attack had begun with fever and delirium.

CASE II.—A man, with an open bubo in his left groin, and who had been ill twelve days; his illness commenced with fever and delirium.

CASE III.—A man, with a bubo in his left groin, which was gradually subsiding. This patient had been ill eighteen days, and had suffered only from fever and this swelling.

CASE IV.—A young man, twenty years of age, was seized with fever and delirium fifteen days previously. He bore a small carbuncle on his right arm, which had already sloughed, but gave indications of a favourable result.

CASE V.—An old man, who had just been taken with violent fever and delirium; pulse strong; pain in the left groin, but without redness, showing the approaching development of a bubo there.

Besides these cases, Mr. Paduan visited, on May 14th, two girls affected with the disease—one, ten years old, had a bubo in the left armpit; the other, five years of age, had a carbuncle on her left arm. The rest of the symptoms were similar to those presented by the foregoing cases.

Mr. Paduan declares that these are instances of plague, but wanting in two symptoms—vomit and diarrhœa—which were common accompaniments of the epidemic plague of 1834 and subsequent periods.

On hearing the above details, Namik Pasha sent a Commission composed of the directors of the Health Department —Mr. Paduan; and Drs. Wartabet and Nazareth, army surgeons; and the Commission was also accompanied by Dr. W. H. Colvill, medical officer attached to the British Consulate at Bagdad. At the same time, his excellency organised a sanitary cordon round the infected locality.

The result of the inquiries of this Commission is not yet known at Constantinople; I shall therefore limit my statement to some further details collected by Dr. Colvill, who made a minute search at Hindieh on the 20th to the 26th May, and from the records of the Board of Health here.

The malady commenced in the beginning of Ramazan (7th January) at a village called Duom, situated between the villages of Hadg Oofee and Hadji Nazr. Duom lost about twenty persons on a population of a three hundred souls; after which the inhabitants quitted this village, and fled towards

extreme thirst; tongue white like a piece of cotton, but not swollen. She could not talk; shook from side to side, and looked like one drunk; eyes swollen and inflamed; sordes collected about the teeth; bowels confined; breathing natural, till just before death. On the morning of the 24th, she vomited three times greenish and white, then eight times blood, and died at midday.

Case II.—Hamedi Ibn Hamud Nasr, aged forty years. On the 18th of May his bowels were opened very frequently, and also on the 19th, passing blood and slime. At night fever came on, which lasted about an hour, then he perspired a little, and then the fever returned. On the morning of the 20th, a red spot appeared upon his right arm, which afterwards turned black, and then the diarrhœa ceased.

Dr. Colvill says, "these two cases will suffice; it is the plague no doubt."

Constantinople, July 17, 1867.

Since my last communication, I have not much to say about the plague at Hindieh. The Board of Health has received telegrammic reports from Bagdad up to the 14th instant, announcing that no fresh case had occurred after the 14th of June; that a cordon sanitaire was established round Hindieh —the infected locality,—with stations at Musseyeb, Hilla, Jweridg, Kiffil, Koufah, and Kerbela; that a quarantine of fifteen days was imposed on all arrivals coming from that place; and, moreover, that pilgrims visiting the shrines of Kerbela and Nedjeff, have been warned not to enter within the precincts of the cordon. On the 7th instant, a party composed of about two thousand pilgrims arrived at Musseyeb from Kerbela, and attempted to force through the quarantine by firing upon, and flinging stones at the guards stationed to protect the cordon, and thus wounded several of them. They met, however, with a firm resistance; their fire was returned, and two of the ringleaders having been hit, the riot was soon quelled.

The first manifestation of plague was reported in the beginning of February; and the intercourse between the inhabitants of the infected locality and the neighbouring towns remained unbroken until the 30th of May, after which date the cordon sanitaire was formed; and yet the malady has not shown any tendency to spread, but has limited its victims to the inhabitants of a small circumscribed marshy district, probably not exceeding thirty miles in circumference. This has therefore given rise to the belief that, if the malady be really plague (as assumed by the medical officers who saw it),

EXPERIMENTS CONNECTED WITH SMALL-POX, INOCULATION, AND VACCINATION.

By JOHN SHORTT, M.D., F.L.S., M.R.C.P.L., etc.,

GENERAL SUPERINTENDENT OF VACCINATION, MADRAS PRESIDENCY.

[*Read November 4th*, 1867.]

First Series of Cases.

During a tour of inspection, having heard that small-pox was prevalent in the station and vicinity of Arcot, I was desirous of transferring small-pox matter from the human subject to the cow, and testing the results. On the 31st of October, 1866, a case of small-pox was said to be in the village of Timmery, eight miles distant. I proceeded thither, accompanied by the deputy-superintendent and staff, and took with me a two-year old bull for the purpose of this experiment. On arrival, we found the subject of the disease (a boy about fourteen years old, of the Pariah caste) covered with small-pox eruption of some eighteen or nineteen days' duration. The eruption about the face had evidently been confluent. and was now scabbing. The trunk and extremities were well covered with pustules, not confluent; these were also scabbing, but the eruptions about the legs and feet presented a pustular appearance. The pustules having lost their central depression, a very sickly, disgusting odour emanated from the patient. Matter was taken from some of the pustules about the feet, and, as the bull had not arrived, a cow-calf about eighteen months old was obtained in the village and inoculated. Eight insertions were made, four about the teats, and two in the inner part of each ear. This was effected about an hour after the removal of the pus from the human subject. Some three hours after, the bull was inoculated from matter that had been preserved in the meantime on plates of glass. Four insertions were made in the scrotum, and one in the inner canthus of the left eye. Some matter was also inserted in a recent scar on the left thigh. It is necessary to state that the bull was extremely wild and unmanageable. He was greatly excited, and had travelled a distance of some fourteen miles before the operation was performed.

Nov. 3rd.—On examination this morning, there is a slight gritty feel about the punctures or insertions on the scrotum of

the bull; but in the cow-calf even the insertions themselves are scarcely perceptible.

5th.—Small circumscribed tumours, varying from a small pea to a four anna piece, covered the trunk of the bull. These indurations are much more prominent about the hind quarters and legs; the glands about either flank, abdomen, and neck are enlarged. The indurations are confined to the skin, well defined, flat, and circular, moving freely with the skin. With this exception, the animal seems in excellent health, feeds well, and dungs regularly.

6th.—The tumours in the bull have increased in number, extending towards the shoulders, neck, and extremities; the several glands have also increased in size, more especially those on either flank, which are about the size of a medium-sized hen's egg. Having asked Dr. S. D. Scudder, of the American Mission, to see the animal, about an hour after, when thrown for the purpose, the greater part of the tumours had disappeared; but they were visible along the inner parts of the thighs, extending down to the hocks. At the same time the off fetlock had a greasy appearance, and was moist with exudation, and, irritated by flies, was covered with several bleeding points. This only showed itself with the tumefactions of the skin, and subsided with them.

One insertion in the left ear of the cow-calf seemed to have vivified, and exhibited a small papule. This was broken, and with the clear fluid that exuded therefrom three children were inoculated:—1. Andecolum, female, aged seven years; 2. Manuel, male, aged ten years; 3. Ponnen, male, aged five years. Six insertions, three in each arm, were made in the girl, three in each arm of the boy Manuel, and three in the right arm of the boy Ponnen, from the calf inoculated at Timmery; and three in the left arm from an inoculated heifer (red), noticed under the second series.

7th.—The papule in the ear of the cow-calf, from which lymph was transferred yesterday to the human subject, has crusted over in the form of a scab. No change visible in the bull.

10th.—The cow-calf, appearing now quite well, the scab having fallen off from the insertion, was discharged. The tumours about the trunk of the bull are decreasing in size and number; the glands are also smaller in size. The general health of the animal does not seem to have been interfered with in any way.

12th.—The tumours have disappeared, and the enlargement of the glands has subsided. The animal appearing perfectly

well, was discharged this day. The girl Andecolum has one vesicle on the left arm broken, and another of a smaller size rising on the right. From the broken vesicle lymph was taken, and reinserted into the failed insertions, after the manner of Bryce's test. One insertion was made in the left arm from lymph in the broken vesicle, and two insertions in the right arm with matter from another inoculated subject named Manuel.*

13th.—Lymph was transferred from Andecolum to the right arm of a boy named Mootoo. The left arm of this boy was operated on the same day with lymph from the boy Ponnen.

15th.—Three tubes of lymph were collected from the girl Andecolum's arm this morning, and forwarded immediately to the Vaccine Depôt, Madras, for trial. Two of the reinsertions of the right arm have formed small pimples, and contain a little pus. She had high fever, with headache, and pain in axilla. Is looking dull. Pulse 116.

17th.—Scabbing progressing. Free from fever. The inoculated puncture in the left arm has not succeeded. Of the two punctures of revaccination in the right arm one has failed; the other is small, contains a little pus, and is inclined to be conical.

19th.—Progressing favourably.

21st.—Removed one scab from the right arm; that resulting from reinsertion simply comprises a small minute black spot. Scabs on left arm still adherent.

25th.—Scabs dropped, leaving scars resembling ordinary vaccination; but at the reinsertions there are simple depressions.

With the lymph from the glass tubes sent to the vaccine depôt the following subjects underwent operation on November 17th:—1. Chellamrah, female, aged one year; 2. Pauhen, female, aged one year; 3. Sylandum, female, aged three years.

Nov. 22nd (sixth day after operation).—Six very good vesicles in each arm. All are said to have suffered from slight fever yesterday.

23rd.—Slight fever again.

24th.—No fever; insertions puriform.

25th.—Pustules progressing.

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* On the 1st November the inner surface of the ears of a heifer; aged two years, was operated on with fresh vaccine lymph from the subject; and on the 6th day lymph from the fully formed vaccine vesicles was transferred to two subjects.

Dec. 8th.—Distinct marks of vaccination. Vesicles ran the usual course.

The following two cases were operated on with lymph taken from the girl Andecolum :—*A.* Sala Bee, female, aged six years, Mahomedan ; *B.* Moothoo, male, aged two years, Pariah.

A.

Nov. 12th—Six insertions were made, three on each arm.

15th (fourth day).—Six small papules rising.

16th (fifth day).—Six vesicles visible, of a full-sized millet-seed, having all the characters of an ordinary vaccine vesicle, containing some clear fluid.

17th (sixth day.)—Reports having had fever last night slightly; warm now ; excited pulse ; pain in axillæ. Lymph was removed to operate on two others, but the quantity was small, and the patient fainted during the operation.

19th (eighth day).—The whole of the six vesicles are surrounded by small black crusts. Nevertheless they contain some clear lymph-like fluid, which was transferred to three other subjects this morning. Reports having had high fever during the night, and is now warm. Two capillary tubes were charged from this subject to-day.

20th (ninth day).—All six insertions are progressing, containing central depressions. In the vicinity of two insertions of right arm small vesicles are making their appearance. Is now free from fever and pain in the axillæ.

21st (tenth day).—The vesicles in the vicinity of one of the insertions appear to have subsided ; those near the second are on the increase.

22nd (eleventh day.)—The insertions have all formed good large pustules, and are maturating. There are three small pustules by the side of one of the large ones in right arm, one small pimple on right cheek, another on the forehead, and four or five on both wrists.

23rd (twelfth day).—Pustules scabbing. Eruptions noticed yesterday are vesicular and somewhat larger. Asked Dr. Scudder to see the patient, and that gentleman was of opinion that the eruptions were of the character of small-pox.

25th (fourteenth day).)—Eruptions on wrists slightly pustular ; those on the face dry. Pustules scabbing.

Dec. 1st (twenty-first day).—The eruptions formed irregular scabs ; those from the insertions have dropped off.

B.

Nov. 13th.—The right arm was operated on from Andecolum, and the left arm from Ponnen (operated on at the same time as Andecolum).

15th (third day).—Five punctures promise to vivify, and have a papular appearance.

16th (fourth day).—The whole six punctures are elevated, and can be distinctly seen.

17th (fifth day).—Good: two broken; contain a little fluid.

18th (sixth day).—Papulæ increasing; broken ones have a slightly inflamed base, others looking well.

20th (eighth day).—Those entire are not very promising; small, with little black spots in centre; do not appear to contain much fluid. Notwithstanding, their contents were transferred to ten other subjects.

21st (ninth day).—Vesicles crusting over. No fever.

22nd (eleventh day).—Pustules scabbing. A few small vesicles, twenty to thirty in number, are interspersed over the face, arms, and legs.

Dec. 1st (nineteenth day).—Well. Discharged.

Second Series of Cases.

Nov. 1st.—Having heard that there was a case of small-pox in the cantonment of Arcot, adjoining the house of Mr. Leigh, inspector of police, we proceeded to examine the boy, aged twelve years, of the Pariah caste. He is well covered with small-pox eruptions, the disease being in a vesicular stage; vesicles pearl-coloured, and of the size of a millet-seed. (Seventh day.) No fever present. He states that he perceived the eruption about seven days ago. The larger and more prominent vesicles were ruptured, and a clear lymph-like fluid taken therefrom, and the following animals inoculated therewith on the spot:—1. A red heifer, two years old; 2. An aged tattoo (pony); 3. An aged jenny (ass); 4. A goat; 5. A dog.

2nd.—The pony happened to be a traveller's, of which I was not aware at the time; consequently, the result on this animal is not known. In these different animals insertions were made as follow: 1. Heifer, teats and ears; 2. Pony, nostrils; 3. Ass, nostrils; 4. Goat, scrotum and inside of the ear; 5. Dog, scrotum and inside of the ear.

5th.—Nothing decidedly perceptible in any of the animals except the goat: the part where the matter was inserted feels hard.

6th.—One of the insertions on the teats of the heifer is slightly raised into a small papule, which is slightly umbilicated. This was opened with a lancet, and the little lymph obtained therefrom was transferred to the left arm of the boy, No. 3 (First Series), his right arm being previously operated on from the Timmery cow-calf.

7th.—Lymph was again this day transferred from the heifer to a boy, aged seven years, of the Pariah caste. There was scarcely any lymph in the vesicle ruptured yesterday. There are one or two vesicles visible over the other insertions. These were also ruptured, and what little came was scraped and put on.

8th.—The heifer operated on was discharged, the sites of punctures having dried up after the lymph required was taken. In the ass it was a complete failure, as regards the inoculation. The dog had two small vesicles in each ear. From these a calf of four months old was operated on; but this animal was eventually lost sight of. Another dog was also operated on from this source. The transfer of one dog to another succeeded, the insertions only showing true indications of the disease.

9th.—Lymph was taken from the scrotum of the goat, and transferred to a child named Chowry Ammall, aged eighteen months. In this child the punctures showed no indications of success on the 12th, consequently the right arm was operated on again with vaccine lymph.

15th.—Two of the original insertions in left arm, which gave indications of failure on the 12th, are now in a papular state, and when examined by a magnifier contain a little vesicular matter; the vesicles themselves are acuminated. On the right arm the vaccine insertions are rising.

16th (eighth day after original operation).—Two punctures on left arm have succeeded well, although the vesicles are small, and contain but little lymph. The reinsertions on the right arm have also succeeded, having the same character, appearance, and size as those in the left. Vesicles of the size of a coriander seed. In the right arm, next the vaccine insertions, one of the original punctures is forming.

17th.—The eruptions on the left arm were broken, and a little lymph removed for transfer to another subject.

20th.—The eruption on left arm scabbing; that of the re-vaccination of right arm has taken on the true character of a vaccine vesicle of the eighth day; and one of the original insertions in this arm, next the vaccine insertion, has formed, and resembles the other in every respect, except size, which is a little smaller. This vesicle was ruptured, and the lymph transferred to another subject. The patient (Chowry Ammall) is reported to have had high fever last night, and the bowels deranged (loose).

22nd.—Modified small-pox eruptions are interspersed over the face and upper extremities of this subject this morning; a few are showing themselves about the lower extremities,

but the eruptions just now resemble more those of prickly heat. The patient is reported to have passed a very restless night. Is cross now, and slightly feverish. Eruptions on left arm, and original one in right, scabbing; whereas the revaccinated punctures in right arm are in a state of maturation.

23rd.—The eruptions on surface and extremities increasing; vesicular on face and upper extremities; papular about lower; constitutional symptoms better. The whole of the eruptions number from 100 to 150.

25th.—This morning, possibly, there are between two and three hundred eruptions, while a few only exhibit the true umbilicated character of small-pox; the majority are oval, bladder-like vesicles, resembling chicken-pox, whilst a portion have acuminated forms, and give a hard and stony-like feel, resembling in this respect the eruption called stone-pox.

Dec. 1st.—The eruptions are scabbing; health better.

Third Series of Cases. (*Raneepet Parcherry.*)

Nov. 10.—Moothoo, aged sixteen, male, caste Pariah. Said to have had fever on the 2nd Nov., and on the fourth morning the eruption made its appearance; few, not exceeding 200 to 300 in number, about the size of coriander seeds, firm, with central depressions, and having a flattened appearance, and of a pearl-like colour. When ruptured, a clear, transparent fluid oozed out slowly; some sickening odour about the patient; free from fever at present. The fluid taken from several eruptions was inserted on the inner surface of the ears and scrotum of a black bull, aged two years, and another jenny (ass) was operated on with the same matter. The insertions in this latter instance were made on the outer side of each nostril by four punctures.

11th.—Insertions in the bull look angry; some indications of abdominal glands swellings.

12th.—Punctures forming: distinctly visible in a papular form, giving a gritty feel to the finger, whilst the centres of the papules are depressed.

13th.—Papules fully formed, with raised margins and central depressions at the insertions only. These were ruptured, and the contents transferred to two subjects this day,—No. 1, Bejana Bee, aged ten, female, Mahomedan; No. 2, Beeban Bee, aged three, female, Mahomedan.

No. 1.

Nov. 15th (third day).—All six insertions are promising.

16th (fourth day).—Three punctures on the right arm, elevated above the skin, and can be distinctly seen.

17th (fifth day).—Only two punctures of the left arm succeeded ultimately, and are now visible.

19th (seventh day).—Two very small vesicles in left arm.

20th (eighth day).—Third insertion in left arm shows indications of rising.

21st (ninth day).—Vesicles appear like ordinary vaccine. These were ruptured, and the matter transferred to two other subjects. Patient complains of pain in the eyes and extremities, and is somewhat feverish now.

22nd (tenth day).—The vesicles broken yesterday are crusting; those on the right arm more papular in appearance; has fever and headache; pains in arms and axillæ.

23rd (eleventh day).—Eruptions on left arm scabbing; right arm vesicular; these were broken, and a capillary tube charged with contents.

25th (thirteenth day).—Free from fever; a few small pimples are scattered over the wrists and trunk of this patient.

No. 2.

Nov. 16th (fourth day).—Insertions slightly elevated.

17th (fifth day).—Two in the right and one in the left arm are raised above the skin.

19th (seventh day).—At present there are only two vesicles promising and visible in the left arm.

20th (eighth day).—Of the insertions in the left arm, one is small and of a conical form, the other two are papular and do not promise well.

21st (ninth day).—Vesicles in the left arm promising; feverish.

23rd (eleventh day).—High fever; fidgety; hands tremulous; papules rising.

25th (thirteenth day).—The face, trunk, and extremities covered with from two to three hundred small-pox eruptions.

Dec. 1st (nineteenth day).—Face and extremities are scabbing. Eruptions about the extremities pustular. These were ruptured, and the contents transferred to a cow-calf, aged eighteen months.

In all there were four small-pox subjects, from whom virus was transferred to the following animals:—Cattle:* Two

* *Vide* tabular statement attached.

young bulls, three heifers, and one cow; one pony; two donkeys; one dog; one goat.

From these cattle there were five successive transfers of the virus to the human subject. The total of subjects successively operated on was forty-three. Of these, six comprised the original transfer from the cattle direct, one from a goat, and the remaining thirty-six were successive transfers from these subjects. Of the seven subjects originally operated on, one absconded, in two cases the insertions showed the true characters of vaccination, and in the remaining four, although the insertions progressed favourably, and in every respect resembled vaccination, small-pox eruptions showed themselves subsequently, and became scattered over their persons; the number of these pustules varying in the subjects from thirty to two hundred. Of these four subjects, the subsequent small-pox eruptions made their appearance in three on the twelfth and in one on the ninth day after the original insertion.

The insertions themselves ran their ordinary course, as is usual in cases of vaccination. In every one of these cases the insertions consisted of six punctures, three in each arm, and in the two subjects who had been free from secondary eruptions, two only in each case vivified, and in both these instances, on the seventh day after original operation, a fresh reinsertion was made in the failed punctures from the same small-pox virus passed through the cow.

Of the reinsertions in these cases, in the first case, only one vivified, a small papule showing itself on the ninth day from original, and third day after secondary, insertion. The papule formed a pustule on the eleventh day after first, and fifth day after second, operation; and the original insertions (the two that succeeded) had maturated, and were now forming scabs.

On the 21st of November (or sixteen days after original, and ten days after secondary, operation) the secondary pustule formed a small black spot, about the size of a large pin's head, whilst that of the original insertion, the scab of one had dropped off, while the other was still adherent.

On the 25th of November (or twenty days after original, and fourteen days after secondary, operation) the two remaining scabs dropped off, the former leaving behind a true vaccine cicatrix, and the latter a mere superficial minute scar on the arm.

In the second case the insertions were made on the seventh day after first operation, from genuine vaccine lymph; the two original insertions in this case had vivified, and were now visible in the form of good vesicles.

On the 15th of November (or ten days after original, and four after secondary, operation) the original insertions had maturated, and were scabbing, whilst the vaccine reinsertions had formed minute pimples containing a small quantity of pus. The patient had vomited, and was suffering from nausea, high fever, pain in the axillæ, and general *malaise.*

16th.—Original and secondary eruptions progressing; vomited twice since last report. General constitutional symptoms are abating.

17th.—All the pustules have scabbed over, while the vaccine scabs are very small; that from the small-pox virus resembles the usual vaccine scab in size.

20th.—Scabs drying.

25th (twenty days after original, and fourteen after secondary operation.—The scabs of all the insertions have come away, leaving good cicatrices on the original, and trifling scars on the secondary.

It will be seen in both cases that the test known as "Bryce's," which consisted in the reinsertion of virus as late as the seventh day, showed itself, and ran its course with the original operation, both in the case of small-pox and vaccine virus.

Lymph from these cases, charged in capillary tubes, and forwarded to Madras,* succeeded well, and there was no secondary eruption; whereas in a third case, operated on at the same time and with the same virus as the above cases, on the twelfth day after operation, some four or five papules were noticed on the face. On the 19th there were from eight to ten small conical vesicles, containing a little limpid fluid. On the 21st the whole surface of the body was scattered over with the eruption, of which there must have been at least fifty to sixty in number. The eruptions which first showed themselves dried without suppuration. A few vesicles on the hands and lower extremities have the characteristic appearance of small-pox—viz., the flat umbilicated look, and raised margin,—while some are hard to the feel, many acuminated, and others forming little bladder-like vesicles. The original insertions in the arm have increased in size, and are now scabbing. On the 28th, or twenty-three days after operation, the scabs from inoculated insertions have fallen away. The secondary eruptions have also scabbed, and a few dropped off, whilst other few passed through the regular pustular stage before scabbing.

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* *Vide* Table attached.

Trial of Small-pox Virus passed through the Cow
Vaccine Depot

Lymph Employed.	Name of Subject.	Date of Operation.	Fifth Day.	Sixth Day.	Seventh Day.
Tube lymph. Nov, 24, 1866.	1. Ponnoosawmy, m., aged 2 yrs.	Dec. 6, 1866.	Six good vesicles rising, three in each arm; in both cases fever last night; no swelling of glands, etc.	Progressing; slight fever.	Progressing; no fever.
Ditto.	2. Ragavah, f., aged 2 years.	Ditto.		Ditto.	Ditto.
Ditto, Nov. 26.	3. Pragassam, m., aged 10 mths.	Dec 7.	Absent.	Absent.	Absent.
Ditto.	4. Chengen, m., aged 7 months.	Ditto.	Six small vesicles; no fever.	Progressing; no fever.	Progressing; no fever.
Ditto, Nov. 27.	5. Sundee, f., aged 1 year.	Dec. 8.	Three good vesicles in right arm; two in left; no fever.	Ditto.	Ditto.
Ditto.	6. Ponnee, f., aged 3 months.	Ditto.	Three good vesicles in left arm; two in right; no fever.	Ditto.	Ditto.
Ditto, Nov. 29.	7. Tharovidian, m., aged 5 mths.	Dec. 10.	High fever last night; six good vesicles are rising in both cases.	Progressing; slight fever in both cases.	Ditto.
Ditto.	8. Elizabeth, f., aged 4 months.	Ditto.		Ditto.	Ditto.
Tube lymph from inoculated calf of 8th day, on Dec. 8, 1866.	9. Meenache, f., aged 3 months.	Dec. 11.	Very high fever last night; great swelling of axillæ and back of neck; six good vesicles rising in both cases.	High fever last night; six good vesicles distinct in each case; swelling of glands continues.	Fever again, not so high; vesicles progressing very satisfactorily.
	10. Moonean, m., aged 1 year.	Ditto.			
Transfer from Case No. 1, operated on with virus from Arcot.	Ellee, f., aged 3 months.	Dec. 12.	Vesicles rising.	Six good vesicles high fever; no axillary tenderness or swelling.	Progressing; slight fever.
Ditto.	Thayee, f., aged 4 months.	Ditto.	Ditto.	Ditto.	Ditto.
Ditto, No. 2.	Vencatee, f., aged 7 months.	Ditto.	Ditto.	Ditto; no fever.	Ditto.
Ditto.	Nursigado, m., aged 3 years.	Ditto.	Ditto.	Ditto.	No fever; progressing.
Ditto, No. 4.	Ellapen, m., aged 5 months.	Dec. 14.	Ditto.	Ditto.	Ditto.
Ditto, No. 5.	Lutchmagadoo, m., aged 5 years.	Ditto.	Ditto.	Ditto.	Ditto.
Ditto, No. 6.	Atchamah, f., aged 3 years.	Ditto.	Ditto.	Four vesicles; two failed; high fever.	Slight fever; progressing.
Ditto, No. 7.	Nagee, f., aged 2 years.	Dec. 17.	Ditto.	Six small vesicles; slight fever.	Ditto.
Ditto, No. 8.	Nagee, f., aged 8 years.	Ditto.	Ditto.	Ditto.	Ditto.
Ditto, No. 9.	Ponnee, f., aged 3 years.	Ditto.	Ditto.	Very high fever; six good vesicles.	High fever; progressing.
Ditto, No. 10.	Putchay, m., aged 1 year.	Ditto.	Ditto.	Ditto.	Ditto.

l received from Arcot for Trial at the
:dras.

GHTH DAY.	NINTH DAY.	ELEVENTH DAY.	THIRTEENTH DAY.	FIFTEENTH DAY.	REMARKS.
·ogressing.	Pustular.	Scabbing.	Crusts almost dry.	Scabs have fallen off.	No extraneous symptoms observed in both these cases, nor was there any eruption other than vaccine, although seen on the 25th day after operation.
Ditto.	Ditto.	Ditto.	Ditto.	Ditto.	
Absent.	Absent.	Absent.	Absent.	Absent.	Result not known.
rogressing.	Pustular.	Scabbing.	Scabs have fallen off.	Well.	Same remarks as Nos. 1 and 2.
Ditto.	Ditto.	Scabbing; one scab has fallen off.	The remainder have dropped.	Ditto.	Ditto.
Ditto.	Ditto.	Ditto; two have fallen off.	Ditto.	Ditto.	Ditto.
Ditto.	Fever still; pustular.	Scabbing.	One scab dropped.	The remainder have dropped.	Ditto.
Ditto.	Ditto.	Ditto.	Three scabs dropped.	Ditto.	Ditto.
ıicles rup-d; fever still.	Pustular; fever.	Ditto.	No fever; crusts dry.	Scabs have dropped.	These cases were seen again on Dec. 29th; the cicatrices were well marked, but no eruption of any kind followed.
ɜr still; vesi- progressing.	Ditto.	Ditto.	Ditto.	Ditto.	
fever; pro-gressing.	Pustular.	Scabbing.	Scabs have fallen off.	Well.	Seen again on the 29th, but there was no eruption; the cicatrices were well marked.
Ditto.	Ditto.	Ditto.	Scabs are nearly off.	Fallen off.	Ditto.
Dit·o.	Ditto.	Ditto.	Scabs dropped.	Well.	Ditto.
Ditto.	Ditto.	Ditto.	Ditto.	Ditto.	Ditto.
Ditto.	Ditto.	Ditto.	Ditto.	Ditto.	Not seen afterwards.
Ditto.	Ditto.	Ditto.	Ditto.	Ditto.	Seen on the 29th, well marked cicatrices; no eruption.
Ditto.	Ditto.	Ditto.	Ditto.	Ditto.	Ditto.
Ditto.	Ditto.	Ditto.	Ditto.	Ditto.	Ditto.
Ditto.	Ditto.	Ditto.	Ditto.	Ditto.	Ditto.
ight fever; rogr·ssing.	Ditto.	Ditto.	Ditto.	Ditto.	Ditto.
Ditto.	Ditto.	Ditto.	Ditto.	Ditto.	Ditto.

In the second series of transfers, almost every case had secondary eruptions, varying in number from thirty to three hundred, these generally appearing about the twelfth day, and having the same character as that just detailed, whilst the insertions themselves progress satisfactorily, like ordinary cases of vaccination. It will be seen in the experiments, that the bull operated on on the 31st of October showed indications of tumefaction of the skin, and enlargement of the glands generally; and out of five animals, this was the only one in which the disease took this irregular form. I am inclined to attribute it to the excitement the animal was under, the distance it had to travel over, crossing a river a mile wide, and having to swim over several portions of it. These circumstances, perhaps, may account for the irregularity. At the same time, the off fore fetlock had a greasy appearance, and was moist with exudation; and, irritated by flies, was covered with several bleeding points. This only showed itself with the tumefactions of the skin, and subsided with them. In the goat also, the insertion on the scrotum with small-pox virus showed a distinct umbilicated vesicle, resembling genuine vaccination, but it was surrounded to the extent of two inches with a greasy moist exudation, and peculiar musty odour. With this exception, nothing else was to be found wrong with the animal. In the cases where matter was transferred from the goat to the human subject, although the insertions ran the usual satisfactory progress, on the twelfth day secondary eruptions to the extent of nearly three hundred showed themselves. The pony having been lost sight of, results were not observed. Of the two donkeys operated on, in one the inoculation failed altogether, and in the other the insertions only vivified and ran their course satisfactorily. In the dog also, the disease showed itself at the insertions only; and when transferred to a second animal, the insertions only vivified and followed their usual course.

Thus it will be seen that, in the lower animals, the insertion of small-pox virus takes on the ordinary character of vaccination, and is capable of being transferred either to other animals or to the human being.

It will be seen in Case II, Manuel, that on the seventh day vaccine matter was inserted at the failed insertions, and it vivified and progressed in the manner described by Bryce, terminating with the originally-inserted virus; and, in another instance, in a subsequent series of transfers, when, on the fourth day, there were no indicaions of the small-pox virus having passed through the cow vivifying in the subject, rein-

sertion of genuine vaccine lymph caused the original insertions, as well as the secondary, to vivify and run their course satisfactorily.

It will be observed from the above series of cases that when the inoculated small-pox matter from the cow was sent to Madras, where there was no small-pox epidemic prevalent at the time, genuine vaccine disease showed itself; but when the same virus was transferred to others at Arcot, where small-pox had been for some time, and was then prevailing, vaccinia, or a modified form of small-pox, showed itself between the ninth and twelfth days after inoculation, and only two subjects inoculated from small-pox matter direct from the cow were free from vaccinia. In all the other original and subsequent transfers at Arcot, vaccinia followed; and in one instance, where four of the insertions of small-pox matter from the cow failed, reinsertions of genuine vaccine matter, as late as the seventh day after primary operation, caused the original small-pox insertions to vivify and run their regular course. This is an interesting feature in this case, as the secondary insertions only showed minute pimple-like formations, and the cicatrices were also equally satisfactory.

Further, the view originally propounded by Jenner, that cow-pox and grease in the horse were identical, is favoured by the fact of grease showing itself in the fetlock of the first bull operated on, and in the scrotum of the goat, and appears to me confirmatory of this view. However, further experiments are necessary to clear the point in a more satisfactory manner.

Observing that in every case a modified form of small-pox resulted from the transfer of the small-pox virus passed through the cow, I now determined as a further experiment and test to pass the virus from one of those modified eruptions in the subject to the cow. With that view, on the 1st of December, 1866, a cow-calf, aged eighteen months, was selected, and virus taken from a modified pustule (that one having all the characters of small-pox) on the arm of a girl named Beejan Bee,* was inserted on each teat, inner surface of right thigh, under stump of tail, and several punctures were made in both ears, taking care to put on a sufficiency of lymph in each of the insertions.

5th.—Nothing perceptible at the insertions, except the puncture on the thigh, which seems slightly elevated, and about the size of a small coriander-seed. Two punctures

* First transfer.

about the tail feel gritty to the finger, but nothing perceptible.

7th.—Seven days after operation. Nothing perceptible, excepting that the puncture on the thigh promises to rise; is reddish, with a brownish scab at the summit, slightly depressed. This was broken this morning, and the little fluid that oozed out was scraped up on the point of a lancet, and inserted into the arms of two children. The punctures about the tail have small dry crusts of blood covering them, and are slightly elevated; those in the right ear are inclined to redden, and are also elevated.

8th.—The papule ruptured yesterday has a circular reddish-brown crust covering it, and the surface immediately around somewhat pink. One of the punctures in the ears has slightly increased, flat at the side, and minute radiating striæ are observed. The size of the puncture itself is about that of a split pea, but not elevated in proportion to its width. There is a second one exactly similar, but of smaller size. These two were broken this evening, and the little clear fluid that oozed was transferred to another subject, and subsequently a capillary tube was half charged, being taken up on the point of a lancet, and passed into the tube. The fluid thus collected was of a straw colour, and was forwarded to the vaccine depôt for trial.

9th.—The papules that were ruptured yesterday are somewhat darker in colour, and have a very thin, light-brown crust covering them. The surrounding skin is somewhat puckered. The health of the animal continues as usual.

12th.—The papule on the right thigh, from which lymph was removed on the 7th, has scabbed; the tumefactions are subsiding. All the others have disappeared.

14th.—The scab on the right thigh has dropped off, leaving a small white mark. The papules on the right ear still retain their brown scabs.

18th.—The scabs on the ear have dropped, leaving small white spots. The animal being considered quite well, was discharged.

Virus from the above case was transferred to the following subjects on the 8th of December, 1866:—1. Aboo, male, aged five; 2. Coopoosawmy, male, aged five; 3. Lutchmee, female, aged a year and a half. In the last two the operation failed. They were revaccinated with matter taken from the successful case on the 18th of December, successfully. The operation was followed in No. 2 by secondary eruptions, but in No. 3 there were none. In No. 1 (Aboo) there were six good

vesicles from the primary operation, characteristic of vaccination; but on the 19th secondary symptoms showed themselves.

Second Transfer.

On the 16th virus was transferred from Aboo (No. 1) to—1. Pareasawmy, male, aged nine. Operation failed; 2. Chinnasawmy, male, aged five. Operation succeeded, and was not followed by any secondary eruption.

Third Transfer.

From No. 2 in the second transfer, lymph was transferred to—1. Ammay, female, aged five. Operation succeeded, without secondary eruptions; 2. Minchee, female, aged four Operation succeeded, but was followed by a few eruptions.

Fourth Transfer.

Lymph was transferred from No 1 (Ammay) of the third transfer, on the 30th December, to—1. Minchee, female, aged six. Operation succeeded, but was followed by a dozen eruptions; 2. Armoogum, male, aged six; 3. Moonosaumy, male, aged six. Operation failed.

Fifth Transfer.

Lymph from No 1 (Minchee) of the fourth transfer was transferred to the following subjects on 6th January, 1867:—1. Mungalutchmy, female, aged five; 2. Jumman Ally, male, aged four. Operation succeeded, followed by secondary eruptions.

Sixth Transfer.

On the 14th January Sydam Bee was operated on from lymph taken from Jumman Ally. The operation succeeded, followed by secondary eruptions.

The experiments were now discontinued. Small-pox continued in the place and vicinity during the whole period.

There were six successive transfers made on thirteen subjects. The vesicles so raised were good, and were exactly like vaccine vesicles; but in these cases, also, secondary eruptions followed about the usual time. Of the thirteen cases subjected to this series of operations, in three only the insertions vivified and ran their natural course similar to vaccination; and in the remaining ten, although the disease succeeded and ran the usual course as the above, still, on the twelfth day, a few secondary eruptions followed in every one of them.

I am indebted to my assistant, Mr. W. H. Boulth, sub-assistant surgeon, and deputy superintendent of vaccination

Salem Circle, for recording these cases for me, and for watching the results in most of these cases, during my absence on a tour; and to Mr. Sub-Assistant Surgeon Lincoln, deputy superintendent of vaccination Madras Circle, for testing and watching the effects of the supply of lymph sent from Arcot to the Vaccine Depôt, Madras.

Statement of Inoculation of Animals.

A. A calf, aged eighteen months, operated on Oct. 31st, 1866. Matter from a pustule in the leg of a patient affected with the disease. The eruption about the face must have been confluent, but is now scabbing. The eruptions on the trunk and upper extremities are numerous, but not confluent, and are also scabbing. The eruptions on the legs and feet in a pustular stage. Punctured on all four teats and in both ears. Result successful.

B. A bull, aged two years, operated on Oct. 31st. From matter from the above, and used about three hours after removal from the subject. One puncture on the scrotum, one in the inner canthus of the left eye, and a little matter was put into a recent wound on the left thigh. Result irregular (*vide* case.)

C. A red heifer, aged two years and a half, operated on Nov. 1st. Lymph (a clear fluid) from a subject with seventh day's eruption. The pocks were numerous, but not confluent; eruptions about the size of a seventh-day vaccine vesicle, and distinctly umbilicated. The fluid was immediately transferred to the following animals. Punctured on the teats. Result successful.

D. A pony, operated on Nov. 1st. Lymph from the above subject immediately transferred. Punctured by the side of the nostrils. Lost sight of.

E. An ass, operated on Nov. 1st. Lymph from Case C transferred. Punctured by the side of the nostrils. Failed.

F. A dog, operated on Nov. 1st. Lymph from Case C transferred. Punctured on both ears. Successful.

G. A he-goat, operated on Nov. 1st. Lymph from Case C transferred. Punctured on scrotum and both ears. Successful.

H. A black bull, aged two years, operated on Nov. 10th. A clear fluid from a boy with seventh day's eruptions. The pocks resembled vaccine vesicles of the same day. The eruptions were from 200 to 300 in number, and with distinct central depressions; pearl colour; when broken a clear fluid slowly exuded from them. One puncture on left ear, three

on anterior surface of scrotum, and one on posterior surface. Successful.

I. An ass, operated on Nov. 10th. Matter taken from the above. Punctured on the sides of the nostrils. Successful.

J. An old cow, operated on Nov. 10th. Matter from a subject. The eruptions were more numerous than in the preceding case, and looked like the little vaccine vesicle of the fifth or sixth day. Pock afforded clear fluid, which was transferred at once. Punctured in both ears. Failed.

K. The old cow's calf, operated on Nov. 10th. The matter used was the same as in the preceding case, and the eruptions bore the same characteristics. Punctured on the teats and in one ear. Failed.

ON THE RECENT OUTBREAK OF PERNICIOUS FEVER IN MAURITIUS.

By Dr. C. F. EDWARDS,

GENERAL SANITARY INSPECTOR OF THE ISLAND.

[*Read December 2nd*, 1867.]

The Island of Mauritius had formerly the reputation of being one of the healthiest of the British colonies within the tropics. Its character has sadly changed. For several years past fever and dysentery have prevailed extensively amongst the Indian population, and the annual rate of mortality has steadily increased until it has attained an average of 37 per thousand in the rural districts, and 48 in the town of Port Louis.

The prevailing fever, a low form of bilious remittent, is supposed by Mauritians to have been introduced by immigrants from Bombay, and is hence called "Bombay fever." There is little doubt, however, that the disease is of local origin, and results from the bad sanitary condition in which the Indians live. This fever sometimes assumes a typhoid and contagious form, and proves rapidly fatal; but usually it is protracted in its course, often complicated with diarrhœa or dysentery, and attended with abdominal congestion, particularly of the spleen, which is almost invariably implicated, thus indicating the malarious character of the disease.

Whether this or any other Mauritian fever ever assumed a distinctly intermittent character prior to 1866-7 is a question yet to be determined. There is certainly no record in the annals of the colony of any epidemic anolagous to that which has lately, in the course of nine months, more than decimated the population of the island. Mauritius has suffered much from several fatal visitations of epidemic cholera, and cholera was the only disease the people dreaded. Although repeatedly warned that fever was becoming far more formidable than cholera, they satisfied themselves with passing the most stringent quarantine laws, and turned a deaf ear to the advice and suggestions of those who advocated sanitary measures as the most effectual means of preventing the spread of either disease. Before attempting a description of the late epidemic,

I will give a brief sketch of the previous sanitary condition of the island, and of certain occurrences which preceded the outbreak, and seem to throw some light on the subject.

At the commencement of the present year Mauritius contained in round numbers about 340,000 inhabitants, 240,000 of which were Indians, the remainder consisting of Europeans, creoles, and Chinese. About a hundred thousand of the Indians are engaged to work on the sugar estates; the rest are entirely free. The sanitary condition of both classes is equally unsatisfactory. The camps are all badly constructed, and many are quite unfit for habitation, the huts being small, dark, frequently roofed with iron or tin, ill ventilated, and badly drained.

The water supply is precarious and often inadequate. Wells have been sunk on some estates and good water obtained; but, as a general rule, the camps are dependent on the nearest river or canal, the water of which is more or less polluted by the Indians themselves, who resort there for the purpose of bathing and washing their clothes. Many of the streams are also defiled by drainage from the camps and the residues from sugar houses and distilleries. It is believed that many diseases to which the Indians are subject, especially diarrhœa and dysentery, may be traced to the drinking of impure water.

The reckless destruction of timber in some parts of the island, although it may not perhaps have lessened the total amount of rainfall, has probably had the effect of diverting the showers, and has certainly tended to diminish the volume of water in many of the rivers. Some which formerly contained a good body of water all the year round are now nearly dried up in certain seasons, or consist of a chain of muddy pools.

The unrestricted liberty which the Indians enjoy after completing their engagements with the planters is a great and growing evil. The whole face of the country is disfigured by their miserable camps, every one of which, from its dirty and overcrowded condition, is calculated in epidemic seasons to become a focus of disease. Large numbers of this class also herd together, like so many pigs in a sty, in buildings of the most wretched description, in the very heart of the principal town, Port Louis, and also add to its unhealthiness. As Port Louis possesses no system of drainage worthy of the name, a good deal of the refuse of the streets, and a considerable portion of the sewage of the town, finds its way into the several water-courses which traverse the town, and,

as all of these streams are more or less sluggish, much of it is retained. The water supply is tolerably good as to quantity, but its distribution is partial. It is brought from a considerable distance by means of canals, but not from very pure sources. One of these canals is constructed of iron, and by the time it reaches the town its water has parted with a good deal of its impurity, and has become tolerably good; but the other two are built of masonry, and their waters, instead of improving, deteriorate as they approach the town, and contain a large amount of organic impurity.

The town contains many other sources of insalubrity. I may mention a large lagoon called the Mer Rouge, situate close to, and connected with, the harbour of Port Louis. Twenty years ago this lagoon was navigable for lighters and other small craft. It is now fast filling in, an extensive surface of mud being exposed at low water, which at times gives off pestilential miasmata. Two of the most polluted of the streams discharged into this lagoon. It may also be mentioned that the railway works, especially those in the neighbourhood of the central station, have tended to increase the unwholesomeness of that part of the town. The execution of these works involved the cutting of a canal for the diversion of one of the rivers from its natural channel. This canal is of considerable length, and almost on a dead level. The soil through which it was cut is of an alluvial character, as is likewise that on which the principal buildings stand. The turning up of such a soil, and the obstruction caused by these works to the free exit of the waters of a polluted river, and their exposure in a stagnant condition to the influence of a tropical sun, could have but one result, and it is not surprising that this was one of the worst affected localities during the epidemic.

In the month of February, 1865, the island was visited by a tremendous fall of rain, which completely inundated the town, effectually stirring up the rivers, brooks, and other reservoirs of filth. As the flood subsided large accumulations of mud mixed with decayed vegetable and animal matter were deposited in the streets, on the banks and at the mouths of the rivers, and in the ground floors and cellars of many houses in the lower parts of the town. An offensive odour prevailed, and an outbreak of fever was predicted, but strenuous efforts were made by the municipal authorities to cleanse and purify the town, and although several cases of fever did occur, they were not of a very fatal character, and the immediate effects of the inundation were not serious.

There was also an unusual amount of fever in the adjoining district of Black River, which had also suffered from the effects of the flood, and it was in this district that the epidemic first declared itself, although not until the following year. The immediately exciting cause appeared to be the cleansing out of a lagoon early in January, 1866. This lagoon is situate in the subdistrict of Petite Rivière, about six miles to the west of Port Louis. Some of the labourers employed in the operation were attacked by fever, and the work was discontinued, but not until large quantities of dark fœtid mud had been thrown up on the banks of the lagoon, where it lay exposed to the sun during five months of an unusually hot and dry summer. It was not until the following July that the mud was carried away to the cane fields for manure. The camp of the "Albion" estate is within half a mile of the lagoon, and that of the adjoining estate "Gros Cailloux," about two miles. Both these estates had been previously healthy, but in the month of February there were upwards of 200 cases of fever in the two camps, many of which proved fatal. This epidemic lasted until June, when the cool weather set in. The earliest cases of fever during this outbreak were of the ordinary type, not distinguishable from common Bombay fever; but subsequently several cases of well-marked intermittent occurred, and these were, to the best of my knowledge and belief, the earliest cases of that form of fever recorded. The disease smouldered during the winter months, but only to break out with increased virulence as soon as the hot weather set in again. The increase of mortality in the district of Black River in the month of January, as compared with that of the corresponding month of the previous year, was greater in proportion than that of any other district of the island. From Petite Rivière, as a centre, the epidemic spread east and west to all the leeward districts of the island, to which it was strictly confined. The malarious poison appeared to be conveyed by the wind, for it was observed that according to its direction so did the number of cases increase or diminish. It seemed to be arrested by the chain of mountains which divides the island, and did not reach beyond a certain elevation. The epidemic attacked all classes of the community, but the greatest mortality occurred amongst the poor and destitute. There is a large creole and free Indian population, who live on the verge of pauperism. They are notoriously an improvident race, and when overtaken by sickness they fall into great distress, as there is no properly

organised system, medical or material, for the relief of the poor. The unusually high price of provisions, especially of rice, the staple food of the people, combined with a scarcity of labour, had tended to impoverish them; and hundreds, perhaps thousands, of this class fell easy victims to a disease which, although not essentially fatal in its character, was so rapidly prostrating in its effects. An immense proportion of deaths, in fact, resulted from want not only of medical assistance, but of the common necessaries of life. The character and symptoms of the disease varied considerably: from simple uncomplicated intermittent to the severest forms of congestive fever. The great majority of cases resembled common ague, the quotidian and tertian forms predominating, although some individuals had weekly, others fortnightly, attacks. Such cases, if taken in time, were perfectly manageable, a single dose of quinine being often sufficient to arrest the progress of the disease: but relapses were very common, and quinine was scarce. If neglected, these cases frequently ran into the remittent or continued forms, with local complications, such as congestion of the liver and spleen, followed by dropsical effusions and fatal results. Not only did cases of intermittent occasionally degenerate into other and more serious forms of fever, but cases of remittent and continued fever were often followed, after recovery from the first attack, by ague fits perfectly well marked in all their stages. As the epidemic declined dysentery became very common, causing a large proportion of the deaths which occurred after the month of June. The symptoms of this remarkable disease were, indeed, so varied as almost to defy description, but all pointed unmistakably to its malarious character.

The localities most fatally affected were those in which the sanitary conditions were the worst. In overcrowded camps and houses whole families were swept off with astonishing rapidity. A deficient water supply was a great aggravation in some places. In two suburban districts notoriously badly supplied, not a family, scarcely an individual, escaped; relapses occurred over and over again, and many of those who recovered from the primary attacks afterwards suffered from dysentery which proved very fatal. The neighbourhood of a marsh in either of the affected districts was observed to furnish a large proportion of fatal cases. It did not appear that marsh miasmata were alone sufficient to generate fever, for most of the marsh land in Mauritius is situate in those districts which escaped the epidemic, but they seemed to

foster and intensify the poison pervading the atmosphere. The question of the contagiousness of the disease was strongly debated before I left the colony, and much conflicting evidence was brought to bear on the subject. The balance of facts, however, appeared to be in favour of those who regarded it as a communicable disease in some of its forms.

The epidemic lasted from January to October, by which time it had died out. It reached its culminating point in April, in which month upwards of 10,000 deaths were registered. The total deaths in the nine months were 38,200, being 11 per cent. of the total population of the island and 16 per cent. of the population of the affected districts. The town of Port Louis suffered to the extent of 26 per cent. of its inhabitants. It is a singular fact that the mortality of the three districts which escaped was absolutely less during this period than the average of former years. From the facts stated, it is submitted that the following deductions may reasonably be drawn :—

First, that the disease was of malarious origin.

Secondly, that the inundation of 1865, followed by two seasons of unusual heat and long-continued drought, were predisposing causes.

Thirdly, that the malarious poison was fostered and intensified in certain localities by their bad sanitary conditions.

Fourthly, that the cleaning out of the lagoon at Petite Rivière was, at any rate, one of the exciting causes.

Fifthly, that the strong resemblance between some forms of this epidemic and the disease commonly called Bombay fever, both in symptoms during life and the appearances after death, leads to the inference that they were modifications of the same disease, the peculiar character of the epidemic having been determined by exceptional atmospheric and telluric conditions.

THE MAURITIUS EPIDEMIC OF 1867.

By R. SIDNEY STONE, L.R.C.P., M.R.C.S.,

RESIDENT ASSISTANT-SURGEON, CIVIL HOSPITAL, MAURITIUS.

[*Read December 2nd*, 1867.]

The late scourge, which has carried off about one in every nine persons in the colony of Mauritius, was, in my opinion, a severe epidemic outbreak of a disease previously endemic to the island—malarious fever. As might be expected, the malady was considerably modified by the epidemic influence at work, and hence varieties of disease due to malarial poisoning were in some instances thought to be complaints before unknown in the place. The indigent condition of a great portion of the population also had its share in producing a disease almost unrecognisable as the typical fever.

The epidemic tendency which has lately shown itself by outbreaks of cholera and chicken-pox, on this occasion took on the malarious form. This influence may be supposed to have been at work soon after the commencement of last hot season—namely, during the months of November and December. It will be seen from the following table that the weather was unusually dry about this time, so much so that only 2·539 inches of rain fell during the months of October, November, December, and January, against 20·166 in the same months last year. This refers to the town of Port Louis, but there was a similar difference noticed throughout the island.

	1862-3.	1863 4.	1864-5.	1865-6.	1866-7.
October	0·589	1·171	0·835	0 818	0·244
November	0·806	0·355	1·900	1·842	...
December	1·129	2·367	1·830	12·093	0·790
January	9.484	2·318	3·270	5·413	1·505
	12·008	6·211	7·835	20·166	2·539

The weather was extremely hot during the latter part of this period and for some months afterwards, the temperature being constantly above the normal standard.

It was remarked that a great many cases of tetanus occurred during the latter months of 1866. It is well known, too, that there were several cases of cholera during December, the germs of which disease had probably been left from a previous visitation; one at least of these cases proved fatal, and I believe there was another also. There was evidence of the presence of malaria from the existence of a case of intermittent and remittent fever. It was notorious, too, how prevalent neuralgia was about this time.

With the exception of a few showers at the commencement of December, no rain had now fallen since the 30th of October, and things were in the unsatisfactory condition described when on the 20th and 21st December some tolerably heavy rain descended. Whether these occurrences were mere coincidences or were in relation to one another as cause and effect, it is probably useless to speculate; certain, however, it is that the epidemic influence or tendency, which some suppose depends on the electrical condition of the atmosphere, found malaria the most convenient and abundant poison for its purpose, as, shortly after Christmas, intermittent fever was becoming epidemic amongst the Indian labourers.

Was the population which was thus open to attack in a favourable or an unfavourable condition to resist it? Unfortunately the greater part were only too exposed to the inroads of an epidemic disease. The partial failure of the sugar crop, and the extraordinary price of rice during the latter half of last year, had necessarily, from the state of want entailed thereby, an effect on the poorer classes prejudicial to sound health. But another and more constant cause of ill-health, and one which must always render any people peculiarly liable to take any disease, was the wretched sanitary condition in which the greater part of the population lived. Their constitutions thus injured by imperfect hygienic conditions, with temperaments naturally indolent and fatalistic, the Indian and lower Creole population presented a very fit soil for the introduction of a morbid poison.

If it be allowed that there was an influence at work tending to cause disease to become epidemic, we shall only require the two endemic conditions—a susceptible population such as I have described, and a specific zymotic poison—for the establishment of a disease which shall "spread rapidly among the people, so as to incapacitate and destroy large numbers of them." The poison in this case I consider to have been

malaria, by which term I mean a terrestrial emanation entirely distinct from animal effluvia.

Holding, then, that the epidemic was one of malarious fever, it will be convenient if I give a definition of what I mean by malaria or the immediate exciting cause of this fever.

By the term malaria, I understood an aëriform terrestrial emanation, requiring for its production a certain amount of moisture, a certain amount of heat, and a surface capable of absorbing both heat and moisture; coexisting often with the products of vegetable decomposition, although not depending on that decomposition, and being entirely independent of animal decomposition.

I will endeavour to point out what I consider the conditions in Mauritius favourable to the production of malaria, and consequently of the fever depending on it.

This island is situate within the tropics, and undoubtedly, therefore, within the malarious zone as it has been called. The leeward shore of the island is sandy, the soil underneath the sand being frequently of an alluvial nature. At the embouchures of the rivers are alluvial deposits, which are occasionally covered with salt water, occasionally with fresh, and sometimes left altogether dry for long periods. These deposits exist in large quantities on some parts of the coast, as, for instance, at the mouths of the Latnaiers and other streams, running into the Mer Rouge, until very lately a stagnant lagoon. There is no doubt that these deposits, which in a tropical climate are well known to produce malaria in abundance, were enormously increased by the great inundation of February 1865; on this occasion large quantities of débris were accumulated at the mouths of the rivers, considerably elevating their beds at their junction with the sea, thus leaving large surfaces sometimes exposed to the heat of the sun, and at other times covered with water. The extensive flooding of the country at the same time, caused quantities of mud to be deposited, which no doubt, under favourable circumstances, was equally productive of malaria. There are along this coast several sluggish pieces of water with low sandy banks devoid of vegetation; such localities as these have been noticed as malarious.

When the south-east trade wind is in abeyance, as it constantly is at the commencement of the year, a more or less perceptible breeze from the north-west is experienced on the leeward side of the island. This condition of things goes by the name of "a Malgash wind." The depressing effect on the system is notorious, and the heat is what may justly be called sultry. Why is this breeze from the sea unhealthy?

Because, I believe, it carries with it the local malaria produced along the coast.*

There are many places both in Port Louis and in the country, where no provision whatever is made for surface-drainage. The water, after tropical showers, instead of running away at once, is allowed to accumulate, saturate the soil, and eventually dry up. This is a process of considerable risk in places where there exists no vegetation to protect the surface from the direct heat of the sun. It is well known that deep ravines are very productive of malaria under certain circumstances. From the extensive destruction of forests in this place, there are a great many old water-courses, and the beds of those which exist as such, instead of being constantly covered with water, are only well flooded at long intervals, so that portions are constantly exposed to a hot sun. In some of the lower parts of the island which have been denuded, the soil deprived of the shelter of vegetation would from a partial wetting and rapid drying present a surface from which malaria might be expected to arise. It must also be remembered that the sugar crop was out and carried for long before the epidemic broke out. The destruction of the forests would besides tend to produce sudden floods and long intervals periods of dry weather, instead of a more equable and healthy distribution of wet and fine.

The marshes, too, no doubt contributed to the generation of malaria, but, as will be seen, I by no means consider that they are the only favourable spots for its production. It is after a long drought that marshes become unhealthy; owing to the partial drying up of their edges; if they are constantly covered with water, they are not dangerous. The cemetery of Petite Rivière as a fever locality is no doubt to be owed to the deleterious action of its long periods of dry weather on the marshy ground in the district. Again, it is an established fact that the disturbance of certain soils is dangerous. Now this has been taking place to some extent of late years in Mauritius during the formation of the railways, and more so at the central station. The excavations in this district were made in alluvial soil, which at present occupies the position previously held by the sea. The laying of the gas pipes may possibly also have contributed to the mischief.

Whether the geological formation of the island and the

* I have frequently heard it remarked that [illegible] not occur so often on clear days, and [illegible] sun, as on "muggy" days, when the "[illegible]" [illegible] favour the theory that insolatio is but a form of [illegible]

nature of its soils would be considered favourable to the generation of malaria, I am not in a position to offer an opinion. These, then, I consider to be the conditions likely to give rise to the production of malaria in Mauritius, and they may consequently be set down as predisposing causes of the epidemic.

The other predisposing causes are those which had a tendency to injure the constitution of the people, and thus render them liable to disease. These are, of course, the defective sanitary state of the town and villages, and the defective hygienic conditions under which the lower classes live. These conditions and their causes are so well known from the able reports on the subject by the general sanitary inspector, that it is only necessary for me here to say that I concur in his views on this all important subject.

The predisposing causes of the late epidemic may thus, I think, be included under three great heads, viz.:—1. Unfavourable weather and time of year. 2. The defective sanitary and hygienic conditions under which a large portion of the community were living. 3. The various conditions favourable to the production of malaria.

The exciting causes being:—1. The epidemic influence. 2. Malaria.

From these remarks on the predisposing and exciting causes of the epidemic of 1867, it will have been seen that most of the evils enumerated might indirectly be traced to two great primary agencies, viz.:—The gradual destruction of the forests, and the accumulation of a susceptible population.

The malarious character of the fever appeared to me to be sufficiently evident from such circumstances as the following:—The paroxysmal nature of most of the cases; the evident value of quinine as a remedy; the increased susceptibility of persons after a first attack; the short period of convalescence under judicious treatment; the presence of the characteristic cachexia, accompanied by splenic enlargement in cases that had been neglected; the frequent after occurrence of attacks of intermittent fever in cases that might have otherwise been mistaken for some continued fever; the great prevalence of dysentery immediately the cool weather commenced, and the violence of the epidemic had abated; and lastly, the fact of the disease having been almost entirely confined to the leeward side of the island, which might be expected to be the chief malaria ground, the poison having been prevented by the mountains and high ground from being blown into the healthy district by the sea-breeze, intercourse also at the

same time being constant. Then, again, although I have no statistics on the subject, I frequently could trace cause and effect in the variations of the wind, and the increase of the fever in certain parts. It was noticed, for instance, that when the south-east trade set in for a few delusive days, the mortality was rather increased than otherwise; something might be due to the greater coolness of this wind; but it depended more, I fancy, on the malaria which had been blown inland towards the mountains, and had gradually accumulated on their wooded sides, being returned back again over the unfortunate districts affected. The wind was usually from some point between north and west.

I have said above, that in my practice, which was almost entirely confined to Indians, the type of the fever at the commencement of the epidemic was intermittent. The first batch of cases occurred amongst the labourers belonging to the Mauritius Dock Company, during the first two weeks of January; they were slight cases generally at first, yielding easily to treatment. Intermittent was by far the most frequent type throughout January, the large proportion of quotidians seeming to point to their being, generally speaking, first attacks. Later on, and more especially during the months of February, March, and April, low remittent fever was in the ascendency, the intermittent, however, still usurping about half the cases. After the hurricane which passed over the island of Rodrigues on April 13th, the effects of which were also felt here, there seemed to be no diminution in the number of admissions, or in the death-rate, but I believe that after this the admissions of all first attacks become less frequent. As the weather became cooler, so the number of admissions from first attacks gradually ceased, and towards the end of May it was seldom that these cases came under notice. The number of admissions and deaths was, however, as yet undiminished, the cases being chiefly those of malarious cachexia and the various accompanying complications, of which dysentery was the most severe and fatal. The mortality from dysentery continued very high throughout June, but dropsy, abscess of the liver, phlegmonous erysipelas, etc., contributed their share. These cases were all stamped by being every now and then attacked with tertian or some other form of ague.

Although during an epidemic it is often difficult to classify the cases accurately under the usual heads, yet in this instance there were several sufficiently pronounced forms of the disease to render such an attempt desirable. I noticed

the following varieties as most constant: there were, however, frequent interchanges of type, especially from the remittent to the intermittent before recovery.

Intermittent fever was most common, I think, amongst sthenic Indians. By intermittent fever I mean a paroxysmal fever with a distinct intermission during which the patient feels perfectly well, with clean tongue, natural pulse, and no headache. Each return of paroxysm is characterised by the usual cold, hot, and sweating stages, more or less complete as the disease advances, the cold stage, however, being rarely if ever absent.

Out of 257 cases occurring in January, February, and March, in which I made a note of the variety, 214 are stated to be Quotidians, 41 Tertians, and 2 Quartans. On referring to the cases it seems highly probable that nearly all the Tertians (and also the Quartans) were originally Quotidians.

Vomiting and delirium were frequent symptoms during the hot stage.

The treatment consisted in preventing the return of the paroxysm at the very first opportunity, by giving a large dose (gr. xv to gr. xxx) of quinine from three to five hours before its expected advent. If the quinine did not act readily and caused head symptoms, gr. ½ of acetate of morphia was always sufficient for the accomplishment of the end in view.

The immediate risk of life in these cases, I always found extremely small; the continued exposure to malaria coupled with frequently repeated attacks, leads, however, to very considerable mortality and permanent disease.

Simple remittent fever was by no means so common amongst the Indians as the last variety, and when present, there was generally a tendency to asthenia. In the few Europeans I had an opportunity of observing, I found that the more sthenic individuals were attacked with the remittent, and the more asthenic, or those who had become acclimatised, with the intermittent form.

By the term remittent fever, I understand cases of general pyrexia, whitish, furred, and quickened pulse; characterised by periods of remission, the exacerbation being not ushered in with a cold stage.

The treatment was generally commenced with five grains each of calomel and James' powder followed by a dose of salts. A diaphoretic and diuretic were often given during the febrile exacerbation, and five grains of quinine every

two or three hours (or in one large dose) during the remission.

These cases generally did well, but had a tendency to take on the adynamic form.

Adynamic remittent fever was, next to intermittent, by far the most common variety amongst my cases. The untoward symptoms were mainly due to famine and neglect.

The characteristics were, general pyrexia of a low form, dry tongue coated with dark brown fur, sordes on teeth, subsultus tendinum, muttering delirium, and seldom any remission for many days.

The treatment generally consisted in administering a mixture containing diffusible stimulants in equal parts of camphor mixture and infusion of bark every two hours, at the same time not neglecting to order good beef tea, brandy, and port wine.

The mortality from this class of cases was, owing partly to circumstances connected with the epidemic, enormous.

Bilious remittent fever, as it is commonly called here, although I think congestive remittent fever would be a better name, was not so common during the epidemic as the other forms. It was, I think, generally confined to sthenic individuals, and probably indicated the reception of a large dose of the poison.

It was characterised by sudden prostration, tongue coated with thick yellow fur, yellowness of conjunctivæ and fulness and pain over liver and spleen, small frequent pulse, constipated bowels, and head symptoms. The attack was frequently ushered in by a stage of collapse with coldness of surface, almost imperceptible pulse, and coma. Suppression of urine was sometimes present, hæmoptysis was also occasionally noticed, and epistaxis was frequent.

This form of fever has been constantly present since my arrival in the island (February 24th, 1865), but during the late epidemic it was generally of a more asthenic type, and often merged by imperceptible gradations into the adynamic remittent form.

The treatment consisted in applying means to relieve the head symptoms. A large dose of calomel and James's powder followed by a dose of salts; diuretics and quinine as early as possible; counter irritation to the liver and spleen. Stimulants were generally required, especially when there was collapse.

The mortality from these cases is very great. They are frequently admitted into hospital in a state of collapse and

die within an hour. The post mortem appearances are then very characteristic, viz. congestion of the contents of the cranium with increased effusion of serum, often of an inflammatory nature, congestion or pneumonia of one or both lungs, enormous enlargement of liver and spleen from congestion, heart gorged with blood, gall-bladder full of black inspissated bile, congestion of kidneys, and general yellow discoloration of body.

This is the variety of fever which gave rise to the report that yellow fever had broken out in Mauritius. The circumstance furnishes another instance of how difficult it is for people to make a distinction between specific yellow fever and a malarious fever having often all the appearance of that disease, although collateral circumstances point inevitably to the distinction between the two.

Intermittent fever occurring in what may be called a chronic form, accompanied by malarious cachexia and some of the sequelæ of malarious fevers, was extremely common towards the end of the epidemic, and still furnishes the hospital with the majority of its cases. The type was very various, tertian being as frequent as quotidian; but other types were common, and more particularly the hebdomadal. The chief sequelæ noticed were dysentery, and diarrhœa, hypertrophy of the spleen and liver, dropsy, and phlegmonous erysipelas of a malignant tendency.

The ague was stopped in the usual manner; larger doses of quinine, however, were required, and the disease being more obstinate, the quinine had to be continued for a longer period. The cachexia was treated by combinations of iron and quinine, and good nourishment. The complications, of course, required their special treatment. The splenic and hepatic enlargements seldom required any different treatment from that employed for the cachexia, although mineral acids were sometimes used in the hepatic affection. Mercury, either externally or internally, was carefully abstained from, as only adding a poison similar, if not more deadly, than that which the system was endeavouring to get rid of. In the cases of phlegmonous erysipelas, early and free incisions were found to be the only means capable of saving the limb and sometimes the life of the patient. But whatever special treatment was required the remedies for the cachexia were steadily persevered in. These patients require long and careful treatment, many never ultimately becoming strong again, and many dying, who should not, for want of that greatest and most necessary of all remedies—a change of air to a non-malarious locality.

There has been, as usual on these occasions, a dispute as to whether the epidemic fever was contagious or not. I think the question resolves itself into the answers to these two questions.

Can remittent fever, when complicated with adynamic (typhous) symptoms, become contagious?

Did typhus or typhoid (enteric) fever exist as an independent disease during the epidemic?

The answers to these questions, to be of any value, would of course have required very patient and unbiassed research during the busy time of the epidemic. My simple opinion is, that the cases were primarily due to the action of malaria, and were therefore not contagious. The fact that the fever was not communicated to a large part of the island, in spite of constant intercourse, shows, I think, that the burden of proof and statement of facts rests with the contagionists?

The causes of the great mortality from this epidemic may be shortly enumerated as follow :—the asthenic condition of the population; the overcrowded, dirty and ill-ventilated houses of the poorer classes; their poverty and the general difficulty of procuring food; their repugnance to meat and stimulants (in the case of Mohammedans) the want of nurses and servants; the insufficient hospital accommodation; the scarcity of medical men; the want of quinine and of persons capable of administering it, the continued absence of the trade wind; and lastly, the great heat of the weather before the epidemic, and the sudden setting in of cold afterwards.

As I have stated that malarious fever was known in Mauritius before the outbreak of the late epidemic, it will be necessary for me to give some corroborative evidence on the subject. The greatest mortality in ordinary years has always been put down as due to "fever." What was this fever? I imagine some of the cases must have been cases of remittent fever, for they evidently were not always due to town influences (*Vide Sanitary Reports*). Then, again, I find that during a period of nineteen years, but not including the last two or three, there were six admissions amongst the troops for remittent fever and one death from that disease. This is a very small proportion, but it is sufficient to point to the presence of malaria, and to make it doubtful whether some of the cases registered as Febris C., were not in reaity Febris R. In my ward of the Civil Hospital I find in the year 1865, four cases entered as "Intermittent Fever," and sixteen as "Bilious Remittent Fever," besides a great many merely noticed as "fever." I believe now that many

of these were really cases of remittent or intermittent fever. Then again dysentery and hepatitis are well known to have had a home in Mauritius for a very long time; these diseases are also equally well known to be very constant companions of malarious fevers, even if there be no more intimate tie between them.

In February 1865, an extensive inundation took place, which, as I have before shown, would be likely under certain circumstances to favour the production of malaria. This apparently happened, for in the following year (1866), I find amongst my cases at the Civil Hospital, 111 entered as intermittent, and 35 as bilious remittent fever. During the latter part of the year the long drought came. Now these two natural phenomena, when taken together so favourable to the generation of malaria, might also be said to favour the late partial failure of the sugar crop, and tend to increase the bad sanitary condition of the town of Port Louis.

In this critical state of affairs the "Deus ex machina" descended, and so potent and sudden was the mysterious influence, that the admissions of intermittent and remittent fever into any section of the Civil Hospital during the month of January (1867), were almost as numerous as those during the whole of the previous year.

THE MEDICAL TOPOGRAPHY OF MAURITIUS,

WITH AN ACCOUNT OF THE FEVER WHICH PREVAILED THERE IN 1867.

By Dr. A. R. BARRAUT,

MEDICAL OFFICER OF HEALTH FOR PORT LOUIS.

[*Read 3rd February*, 1868.]

Mr. President and Gentlemen,—The keen and lively interest which you have exhibited in the Mauritius epidemic, and the flattering manner in which my presence among you was noticed on a former occasion, render it a matter of pleasing duty on my part to redeem the pledge I then made of giving you the results of my observations on one of the greatest visitations which has ever affected any country on the surface of the globe. And certainly, nowhere than in the midst of your Society, which stands so prominent in the investigation of the laws which influence disease, could I find a better opportunity of attempting to point out the curious phenomena which were conspicuous in that epidemic, and the discussion of which cannot but throw some light in the elucidation of some of the facts connected with that great and mysterious entity which we call fever.

The accounts which have reached England seem to be so incomplete, and the descriptions of the origin and progress, and even nature, of the disease, which has spread such desolation in every rank of the population of Mauritius, give such a faint notion of the real extent of our misery, that I trust it will not require any apology on my part, in order to make the details in which I am going to enter more fully understood, if I give you a short topographical sketch of the island in general, and of Port Louis, its chief town, more in particular. In so doing, you will be better enabled to arrive at a correct conclusion respecting the physical condition of the country, and the peculiar agencies which had been at work for years past, and contributed in no small degree to render one of the most beautiful colonies of the British Empire, and one of the healthiest climates in the world, a nidus of disease and a source of terror to its inhabitants.

Situated in the Indian Ocean, in the parallel of 20 deg.

9 min. 43 sec. of south latitude, and 55 deg. 7 min. 30 sec. of east longitude, Mauritius was discovered in 1507 by the Portuguese under Don Pedro Mascarenhas, who called it "Cerne"; but it seems that they did not attach much importance to the new country, for they soon left it. In 1598 it was visited by some Dutch navigators, under the command of Cornelius Van Neck, who gave it the name of Mauritius, from Maurice de Nassau, the then Stadtholder of Holland. The period of the Dutch occupancy was not long, for they abandoned the island in 1712. On the 28th of September, 1715, the French, under Guillaume Dufresne, came to take possession of it, and called it the Isle of France, by which name it is known up to the present day. In their hands the colony gradually rose in importance; and under the governorship of the celebrated Mahe de Labrudonnais, it soon acquired a great renown for its commerce, healthiness, and produce (chiefly spices). The filibustering expeditions which started from the little island caused, however, such ruin and havoc to the British settlements and shipping in India, that the conquest of the island was determined upon and effected in 1810. Since then it has remained a British possession. It will be out of place to relate here the many critical days through which Mauritius passed before it attained its present position. The emancipation of the slaves which the energy of the great Wilberforce procured at last for the British colonies, seemed to slacken for a time the progress of the new settlement; but gradually, with the introduction of free labour from India, the staple production of the country, sugar, soon assumed vast proportions. Its commerce extended its ramifications far and wide, and thousands of ships flocked into its magnificent harbour to bring the produce of the different quarters of the globe, or to seek a refuge against the cyclones which seem to be the apanage of all tropical countries.

The island cannot boast of any considerable magnitude. Ovoid in form, it extends over 70 miles from north to south, and its breadth from east to west is 30 miles. Its superficial area is of 432,680 acres, and its soil gradually rises almost uniformly from the shore to a point 2067 ft. above the level of the sea, which is called the "Piton du Milieu." This point is not, however, the highest in the island, for there is another in the Black District which has an elevation of 2902 ft.

Mauritius is essentially a mountainous country, and the sharp outline of the different ranges, which stand out in bold

relief in its fine azure sky, gives to the island an appearance of picturesqueness seldom to be surpassed. It is rather difficult to describe these ranges, but for the sake of convenience, I shall confine myself to two of the principal. The first, which serves as a natural frontier to the town of Port Louis, and seems to emerge from two great centres, the "Pouce" and the "Peter Both," extends from east to west as far as "Morne Brabant." The second chain has a south-easterly direction, acts as a boundary to Grand Port, and comprises the "Terre Rouge" mountain, the "Camisard," the "Creolis" mountain, and by continuation the "Bamboo mountain." These two ranges, by means of intermediate and secondary hills, seem to encircle the island with a belt of basalt. By their conformation, and especially by the wide space which is left open in the centre of the island, they seem to give additional evidence to the theory which ascribes Mauritius, and its sister island Reunion, to be two portions of a vast continent probably connected with Madagascar, and which would have been rent asunder by one of those subterranean convulsions which took place long before our age. This theory, however hazardous, is likewise supported by the existence of a volcano still burning at Reunion, and enormous blocks of lava occasionally to be encountered in the lowlands of Mauritius.

The country is beautifully intersected by rivers, or, more properly, large streams, the water of which is excellent, and without an undue proportion of earthy salts or organic impurities. Among the principal are—the Grand river, which supplies, in great measure, the wants of the town of Port Louis; the Grand river, south-east, the source of which lies at the foot of the Camisard; the River of the Post, which separates Grand Port from Savanne; the river "La Chaux," which supplies the town or village of Mahebourg. To these rivers we must add a few others of no inconsiderable magnitude, which traverse some of the most important sugar estates. In the centre of the island is a vast lake, which is called the "Grand Basin," and is supposed to be the crater of an extinct volcano. This vast sheet of water, which is never dry, of high specific gravity, in perhaps the most elevated part of the country, is unrivalled for its picturesqueness and beauty. Surrounded as it is by trees of enormous dimensions, rising one above another, with foliage of the most exquisite shapes and colours, it shows to its fullest extent the exuberant vegetation of a tropical clime. Ferns and orchids sprout from every nook; wild flowers send forth

their sweet fragrance; mosses of the most delicate hues spread their velvety covering on the weather-worn rocks; and sometimes amid the thickets naturally formed by the thousands of climbers, which twine in every direction, the startled deer, surprised in their solitude, show their curious shaped antlers, and give additional beauty to the scene.

Encircled by reefs which project far and wide into the sea, the shores of the island, with few exceptions, are all sandy, steep towards the south, and more level in other directions. At short distances are coral islands, which in their slow but progressive formation, seek a sort of natural connection with each other, and which may probably, in times not far distant, enlarge the superficial area of the island. At the entrance of Port Louis harbour are several lagoons, which show the tenacity of the hard-working zoophytes. Flat and Gabriel islands, situated at the north-eastern part of Mauritius, no doubt owe their origin to the labours of these indefatigable artizans.

In several places around the coast are creeks or bays, which can admit large ships. Tombeau Bay, so much known from the immortal author of *Paul and Virginia*, is both beautiful and safe. In 1864, Admiral Keppel moored there two men-of-war, the *Brisk* and the flag-ship *Forte*, of three thousand tons. Grand Bay to the north and Jacolet Bay to the south of the island, are, if not larger, equally safe. Port Louis and Port Victoria, at Mahebourg, are, however, the most important for the shipping. The former is situated to the south-west of Mauritius, and by its dimensions, its safety, its commodious docks, and the advantages which it offers in every respect, has acquired a far-famed celebrity. Placed in the line of passage of vessels bound to the eastward, and in the track of the cyclones which pervade the Indian Ocean, its geographical position has rendered it a favourite place of resort to the shipping of all nations, whether for reference or refuge. It is certainly a matter of regret that Mauritius could not be made a naval station for the ships of Her Majesty. The enormous sums which have been lavished in the building of fortifications would have amply sufficed for a few men-of-war, far more useful in times of emergency than a large body of soldiers.

Port Victoria, on the other hand, though important during the French occupancy, has dwindled into comparative insignificance, on account of the difficulty which ships experience in sailing out, the wind being generally from the south-east during the greater portion of the year.

The seasons are not so distinctly marked that we can give them the division which is usually adopted in Europe. Our winter is nothing but spring, which lasts six months; and our summer, which constitutes the other portion of the year, is called the rainy, or rather the hurricane season.

This latter season begins properly on the 1st of December and ends on the 1st of April. It is hot, moist, and stormy. Hurricanes are generally plentiful, and sickness prevalent.

From the 1st of May to the 30th of November, the temperature is mild and agreeable. It is the time for amusements, theatres, races, and balls, and also that for cutting down the canes and making sugar.

From numerous observations, continued over a number of years, it is found that the barometric tension seems to be higher in July and August, and reaches its minimum in January and February: high with southerly, and low with northerly winds.

The thermometer, on the other hand, reaches its maximum in January and February, and its minimum in July and August. Our mean temperature in summer is generally 84 deg., but last year, in January, the thermometer stood for several days at 92 deg. in the shade. In the cold months, the temperature is about 76 deg. in Port Louis, but at Curepipe, which is nearly 1800 ft. above the level of the sea, the thermometer sometimes falls as low as 54 deg. in July and August.

It seems from observations taken a hundred years ago by M. Lillet, an officer of the French Royal Engineers, that the temperature has not varied much, though the island has been stripped—too recklessly, I must own it—of the forests which formerly covered its soil; but there is certainly a diminution in the rainfall, a fact which must tell in favour of the theory that they induce rain, and consequently favour moisture.

In 1790, in the month of January, with a barometric pressure at 28 deg., there fell 6·3 in. of rain; whilst in January of last year, barometer at 30 deg., there were only 3·2 in. of rainfall.

The island is divided into nine districts, of which Flacq, Rivière du Rempart, Pamplemousses, and Port Louis, being freely exposed to the south-east trades, are called Windward Districts; Grand Port, Savanne, and Black River, Leeward Districts; and Plaines Wilhems and Moka, being in the centre of the country, are called Midland Districts.

The estimated population of Mauritius at the end of 1866 was about 342,000 inhabitants, thus distributed:

Windward Districts	210,000
Leeward	80,000
Midland	52,000

That population is mixed and composed of many heterogeneous elements; for the sake of convenience, I shall state that a little more than one-fifth compose the general population (Europeans and Creoles, either white or coloured), a little less than another one-fifth the ex-apprentice population, and that the Indians make up the remaining three-fifths.

Taking in consideration the extent of the island, we find there is about an acre and a-third of land for each inhabitant, but the population is far from being thus uniformly distributed: there are vast tracts of land, which are either laying waste and uncultivated, or kept as forests for the hunting of deer; while there are some parts favourable for culture or trade which attract considerable masses of population. Port Louis, for example, had more than 80,000 inhabitants at the beginning of 1867.

Port Louis is built in a valley which to all appearances is of alluvial formation, its area consisting of nothing less than immense deposits of vegetable earth congregated together by the work of ages or the labour of man. No sedimentary or metamorphic rocks are found in its substance, and the total absence of regular strata unmistakably points to the true volcanic origin of the island, and leaves no doubt that the bed of Port Louis is nothing else but the matter washed down from the higher lands mixed up with the detritus of the coral reefs along the coast.

From east to west a beautiful chain of mountains encircles the town with a belt of basalt; these mountains temper the violence of the trade winds, and when they were covered with trees, they kept up an agreeable temperature in the vast valley which constitutes the town. At the present time the reverberation of the sun on their barren slopes seems to increase the intense heat for which the months of January and February are generally remarkable.

First, and at the north-eastern extremity of, and immediately to windward of the town, there is the Roche Bois Cemetery where thousands of bodies have been buried; the establishment of the "Engrais Mauricien," where the most disgusting products of the sewage of the town are allowed to putrefy in large open cisterns freely exposed to the air; the establishment of Mr. Garbert, who introduced peat charcoal in Mauritius for the disinfection of fæcal matters, and managed

to get a patent for his process; and, immediately behind, the slaughter-house, where all the cattle destined for the maintenance of the inhabitants of Port Louis, and some of the surrounding country districts, are daily killed. To complete this part of the picture, the "Mer Rouge," a sort of creek or lagoon, receives direct the liquid, and sometimes the solid, refuse of the "Engrais Mauricien," the filtrations of the Roche Bois Cemetery, and through the Bassin Saucet the residues of the slaughter-house and the washings of Mr. Garbert's bags and tubs. These products, which have been accumulating for years past, prevented from being wafted out into the open sea by the rising fringe of coral reefs, constitute large areas of soft mud which decompose in the sun, and from which clouds of mist may be seen arising at the first dawn of day. As we come nearer, we meet with huge heaps of rubbish resulting from the scavangering of the town, partly burnt and partly undergoing a slow fermentation. Nearer still we come to the Albion Dock, a grand conception in a commercial point of view, but a pitiful result for the health of the town, for the brook which once served as a natural outlet for the eastern part of Port Louis, has been diverted from its course to enlarge the area of the dock, and can now scarcely empty itself into the sea. Nearer again, we find the Fanfaron, which formerly held ships of war and large East India merchantmen; the Immigration Depôt, with its floating population of upwards of one thousand Coolies at times, the civil and military hospitals, and the Bank's Hall, or Lascars' quarters of the Port Department.

If from the eastern we pass now to the western part of the town, following the sea-shore, we meet with another intramural cemetery, which has been three times enlarged; the establishment of the Gas Company, with few or no outlets for its residues; the village of Cassis, situated in a marshy and sandy ground; immense masses of scavenging still undergoing slow putrefaction, tanneries, and Indian camps belonging to trading companies in the town.

Then the Caudan (the Royal Engineers' quarters); the railway, placed, as it were, in the mud, accumulated from time immemorial at the deltas of the Pouce and La Butte streams. Then, following the edge of the mountain slope, innumerable huts, belonging to the remains of the ex-apprentice population, and surrounded by insuperable hedges of cactus, which act as receptacles for the filth of that part of the town; Champ Delort, the Tranquebar, the Pouce Valley, and Champ de Mars, where by the side of elegant villas can

be seen thousands of huts, inhabited by Indians heaped upon each other, grovelling in filth, and preserving, with a religious enthusiasm, the primitive habits of the countries where they were born.

In several parts of the town there existed formerly numerous streams, or, as they are called in Mauritius, "ruisseaux," made by nature to convey to the sea the surplus rain-water which used to fall over the town at certain seasons of the year. But this was not their only use. They constituted also the main natural drainage channels of the town; and as long as their waters followed an inclined plane, and went straight to the sea, the process of purification went on, unobserved, unheeded, and, I may say, unseen.

These streams, as the population of the town kept on increasing, were polluted: they became the receptacles of ordure, and gave off the most offensive emanations. At this juncture of affairs, the deplorable effect of trusting to persons with little or no engineering skill was most conspicuous. Instead of applying to this state of matters the appropriate remedies, others were resorted to; some of these streams were blocked up and filled with large masses of stones and heaps of rubbish, products of scavengering, and enormous quantities of cactus which had been cut down for the purpose of sanitary improvement; others, such as those of Labutte and Pouce, were diverted from their natural courses, and made room either for the railway, or the Mauritius and Albion Docks.

The consequences of this mode of procedure are every day becoming more apparent. As soon as the rainy season sets in, the fears of inundations are entertained, and the disastrous effects of the torrential rains of February 1864 are still vivid to every mind.

Besides the manifest evidence that there is no direct outlet for the surplus rainfall, there are also most palpable proofs that the sewage of houses stands in the same predicament. Few houses in Port Louis can boast of well constructed drains. So little regard seems to be paid to level or incline, that large cloacæ, full of stagnant and fœtid water, are seen in courtyards, and show in innumerable instances the most disgusting products of sewage. These cloacæ are, however, unfortunately not confined exclusively to the courtyards of houses: they may be seen in some of the most frequented streets of Port Louis, and in the suburbs, and localities where great numbers of Indians are congregated together. They offer a most unpleasant sight.

The accumulation of the surface drainage of houses and the surplus rainfall and pipe-water has resulted in the collection of an enormous quantity of fluid, which lies stagnant in the subsoil of the town, and which, for the reasons I have mentioned above, must keep the soil in a continuous state of moisture.

The diseases which are generally prevalent in Mauritius are measles, chicken-pox, and common ephemeral fever in the summer, and remittent or typhoid fevers at all seasons.

It is difficult to give even an approximate ratio of mortality from these diseases, as many are registered under different designations, and certificates of the causes of death are not required by law. The various epidemics of cholera and the last visitations from fever have told heavily on the population of Mauritius, and very few years would suffice to exterminate all its inhabitants if the rate of 48 per 1000 in ordinary times is allowed to go on.

The bilious remittent fever of India, so well described by Morehead, Sir Ranald Martin, and other writers on Indian diseases, is generally ascribed to a malarious origin, and held to be not communicable from person to person. This notion, which undoubtedly holds good for India, has been found untenable for Mauritius, where its communicability, its infectiousness, and its appearance in localities entirely free from malaria have been traced with the greatest discrimination. Whilst some medical men in the island are anxious to give the disease a place in systematic nomenclature, the population of Mauritius commonly term it the Bombay fever, from the notion that it was originally imported from that presidency. In 1863, at the request of the acting governor of Mauritius, I undertook some researches on the nature of that fever, and the results of my observations were embodied in a report which I submitted to Government in the early part of 1864. I confined myself to three points: 1. The etiology; 2. The symptomalogy; and 3. The treatment of the disease.

I found that the existence of the fever could be traced in the colony as far back as 1838, and that it was chiefly localised among the Indian population, especially in the wretched huts which constitute the camps of sugar estates. Slow at times in its progress, it broke out on several occasions with remarkable virulence, and caused great mortality. But the belief that it was an exclusively Indian disease was so universal, that though several instances took place among the general population, it attracted but very little attention.

By a comparison of the symptoms which were seen in Mauritius, and those which were described as occurring in India, most notable distinctions were found which showed certainly some signs of similarity, but pointed in an especial manner to a difference not only in degree, but also in kind. It was the difficulty which I experienced in finding an appropriate and, at the same time, a legitimate name for it, that I was induced to retain its popular appellation of Bombay fever, however unscientific that name could appear. The characters which it exhibited were not so marked as to make it a specific fever; but it seemed to me to partake so much of the true nature of typhus, that I described the Bombay fever as a modification of that disease. In fact, when it was introduced in the island of Reunion by a batch of Coolies arriving from Calcutta on board the Eastern Empire, the French authorities were unanimous in calling it contagious typhus. It stopped soon in that colony, because energetic measures were adopted with regard to the sick; but it continued to extend its ravages in Mauritius, because for us it was the pure bilious remittent of India, a malarious fever, and consequently not communicable.

The reasons which led me to consider the Bombay fever as a modification of the true typhus were the following:—

1. The suddenness of the attack. An individual in robust health is stricken down generally after a full meal; his skin gets hot, his face flushed, conjunctivæ congested, eyes suffused, the pulse rises considerably, the heart's action becomes excited, the respiration hurried, and the pupils contracted. When not immediately attended to, these symptoms soon give rise to others of a more distressing nature; delirium sets in, sometimes low and muttering, sometimes loud and noisy; the face and extremities become livid, a profuse sweating comes over the surface, with cold breath and a weak, fluttering pulse. Then there is a feeling of intense prostration, pain in the back and loins, the muscles are set into twitches, tendons are contracted, and after twenty or thirty hours the poor sufferer dies in a state of collapse resembling that of cholera, with the exception that in the latter disease the functions of the brain remain unaltered, whilst the patient in Bombay fever loses his consciousness from the very first commencement of the attack.

2. The continued type of the fever. Whilst the bilious remittent of India is characterised by remissions of some kind, the Bombay fever seldom offers any of these.

3. The breaking out of the fever at all seasons, and under

the same conditions which are favourable to the propagation of typhus.

The bilious remittent is known to be prevalent in localities where malaria undoubtedly exists, and only shows itself at certain times of the year, when the elements of malarious generation are rife. In Mauritius, on the contrary, the Bombay fever will break out in the cold and bracing winter, or in the hot and sultry summer, in low and humid grounds, as well as in the dry and elevated parts of the island, where cholera never showed itself. It has no power of selection; and, unlike yellow fever, it does not attack new arrivals, but dashes at the old residents of the country, sometimes selecting for its victims one class of Indians in a camp, playing the greatest havoc with them, and leaving the others in a state of comparative immunity; taking the hale and strong instead of the weak and effeminate; leaving the Bombay Coolie, fond of vegetables, or the abstemious Calcutta, to strike down the vigorous Madrasese, so remarkable for his European propensities, his free living, and great relish of animal food.

4. Bilious remittent fever, as all malarious fevers are, is amenable to treatment by quinine; the Bombay fever of Mauritius is not. Hæmorrhages are frequent with us, whilst in the true bilious remittent that phenomenon is generally absent.

To sum up: the tendency of the disease to run a certain course (fifteen to twenty-three days); its continuous type, its communicability from person to person, so well traced, not only in Indian camps, on board Coolie vessels, or in gaols, and its breaking out when there were conditions of insalubrity favourable for its spread, led me to the conclusion that we had to deal with a form of typhus fever, as deadly in its effects, and modified so far as there was no eruption to be seen on the patients.

The existence of a formidable disease being thus, I trust, amply established, it is easy to imagine that every year the number of cases increased amazingly; and, as I said before, the returns of mortality, if kept on the same scientific principles as those adopted by the Registrar-General of England, would tell volumes on that score.

In January, 1866, the proprietor of the Albion estate, situated in the district of Black River, determined on using for manure the mud which had been accumulating for years past in some marshes close to the sea-side; and the quantity of decomposing animal and vegetable matter which was exposed to the sun

on that occasion was indeed very large. Whilst the operation was being carried on, numerous cases of fever declared themselves, not only among the Indians engaged in that work, but also among those of the neighbouring estate, Gros Cailloux. The type of the fever was generally referred to that of the common Bombay fever, and I am not aware that any other than the usual measure of unroofing and burning the huts where the Indians lived was adopted. It would have no doubt advanced the interest of science to have ascertained whether fever had existed in the camps before the clearing up of the marsh, or whether the latter was the exciting cause of the outbreak. I am alluding to this point here, because it was believed that the epidemic of Port Louis was due mainly to that cause, and to the inundation of February 1864. It is impossible for me to join in this opinion: the details I have been laying before you, and the results of my practice, lead to a different conclusion; and, besides, the numerous cases I attended in the several classes of the community in the town, long before that period, led me to consider more than once that number a fair quota of an epidemic slowly preparing itself for an outbreak.

Since October 1866, the weather had been getting hotter and hotter, and in January 1867 the heat was so intolerable, that fears of a severe drought, which would have ruined our plantations, were entertained by many. The high price of our staple food, rice, had weighed heavily on the poorer classes of society, and a feeling of distress betrayed itself everywhere. The rainfall was very scanty, and the wind generally blowing from the westward, seemed to strike against the range of mountains which surround the town, and prevent all terrestrial radiation. The electric tension was at its minimum, and the leaden sky which hung over the town was overpowering.

It was at this time that hundreds of Indians, whose contracts of service had expired, and finding no employment on sugar estates owing to the financial difficulties of the planters, flocked into the town, or settled themselves in the wretched huts used as dwellings by that part of the population in the suburbs of Port Louis. These huts are made up principally of the wood of cases that come from Europe with merchandise, covered with tin, and generally never more than four or five feet in height. Some enterprising Indian owns sometimes a dozen of these huts, and derives considerable profit by underletting them to his comrades. Attempts at a regular description of these abodes are here vain, and one must see

in order to form an opinion of these places, where the most primitive principles of salubrity, and even of self-preservation, give way to degradation and unrestricted licentiousness.

At that time, also, the sanitary state of the town was very imperfect. The gutters were replete with all the sewage matter of houses, which remained there stagnant, covered with green vegetation, and exhaling rank and mephitic odours. The soil itself was parched and dried up in several places, but moist and damp where animal and vegetable matter had been left decomposing. To add to these, the "inodore" carts were moving in every direction, and at every hour of the day wafting in the air the gaseous emanations of their contents.

Such was the physical condition of Port Louis, when, in the beginning of January 1867, the indulgence in the festivities of the new year, resulting in errors of regimen, changes in food and drink, especially of adulterated fermented and spirituous liquors, brought on a remarkable feature in the medical constitution of the town. Cases were not, as heretofore, isolated or confined to peculiar localities, but they broke out in every direction with wonderful rapidity, attacking whole families, reducing them at once to distress and misery. The concomitant symptoms of Bombay fever were no longer recognisable; the type of the disease was intermittent, and a different mode of treatment became absolutely necessary.

The new disease, for such it was for Mauritius, generally began by a frontal headache, pain in the loins, and weakness in the limbs; there was a feeling of lassitude and prostration, with frequent yawning or retching; then the tip of the nose and hands began to get cold, the features shrunk, the skin got bluish, the teeth chattered, and the whole body was put in a state of tremor. After some time these symptoms subsided, a general glow came over the surface, the membrane of the nostrils, which felt dry, became moist, and the skin quite warm; perspiration soon followed, urine was voided, and generally fæces passed. In most instances, the patient felt quite well, and inclined to eat. These symptoms were generally noticed at all periods of the epidemic, and in all classes of the population, and they were especially noticeable after hot days with westerly breezes, and followed by cold nights with the wind from the southward.

This description is, however, applicable to the first attack only in a mild form. If remedial means in the shape of antiperiodics were not resorted to at once, the attacks were

sure to succeed each other with great rapidity—to such a point that the system soon became completely saturated with the fever poison, and a general cachexia was established. The skin got very sallow, the conjunctiva of a deep yellow, the liver and spleen enormously swollen, the tongue covered with a thick, brownish fur, and sometimes general anasarca or partial dropsy was seen. Constipation was obstinate; the urine deep-coloured and scanty, tinged with bile, of high specific gravity (1.020), and almost in every case albuminous. At this stage of the disease so much care, attention, and good nourishment were required to keep up the vital energies, that death was the inevitable consequence among the poorer classes of the community.

It was worthy of remark that, pain in the right hypochondriac region indicated the free use of purgatives, whilst pain in the left hypochondrium showed that the spleen was the organ primarily attacked, and indicated the immediate employment of quinine. It was by paying attention to these points that I was enabled to get most of my cases through with no little amount of success.

The complications which were generally noticed were: enlargement of the liver and spleen, dysentery, hæmaturia, mania, œdema of the feet and scrotum, and in some instances, paralysis of the lower extremities. As for the types of the disease, I found the proportion of quotidians ranging between four to five, tertians two to five, and quartans one to five. About seventy-five per cent. of my cases had a recurrence of the disease after a considerable interval of perfect freedom, that interval varying between a week and a fortnight, or even two or three months. These recurrences happened generally after obstinate constipation, and though, in some cases, I could not detect any signs of enlarged liver or spleen, they seemed to be chiefly due to a mechanical obstruction of the bile-ducts, for the quantity of bile vomited in some instances was enormous.

In *post mortem* examinations, the following appearances were found in cases in which death occurred rapidly: an intense yellow colouration of the skin, and immense enlargement of the liver and spleen,—the latter organ reduced to a state of pulp. In cases where cerebral symptoms had been prominent, the vessels of the dura mater were distended, the surface of the brain studded with minute reddish spots; and in the instances where convulsions had taken place, there was a quantity of yellowish fluid in the ventricles of the brain. In almost every case which I examined, the gall-bladder was

full of a thick, viscid fluid, the liver hard, and evidently paralysed in its functions.

From an examination of the facts which I have brought before you, gentlemen, you will see that in the fever of Mauritius two forms of disease were apparent: one, the endemic, or ordinary, which showed the concomitant phenomena of typhus fever, and which we owe, no doubt, to overcrowding, filth, and the pestilential emanations of our two cemeteries; and the other, the true intermittent, which mainly owed its origin to the quantity of water concentrated in the subsoil of the town of Port Louis, and which, coming in contact with the immense masses of vegetable and animal matter which had been heaped up in several localities, produced the marsh-poison in its genuine form. In other words, the endemic fever of Mauritius has both a vegetable and animal poison in its production, whilst the epidemic of 1867 owed its origin to a poison of an entirely vegetable nature.

It is not my intention here to enter into a geological dissertation to prove that Mauritius has undergone in some parts of its crust a process either of elevation or of subsidence: the elucidation of that point would necessitate researches far beyond my power. But one fact, which I cannot withhold from stating, is, that the coral reefs which are springing up all around the coast with wonderful rapidity, raise a formidable barrier to the flow and ebb of the tide, and prevent the organic impurities which are suspended in the liquid sewage of the town from being carried away into the open sea. Large surfaces of mire putrefying in the sun are thus laid bare, and become fresh and additional sources of malarious generation So then, however willing I might be to ascribe the heat which we have in Mauritius to the stripping of the forests and the denudation of our mountain slopes; however disposed I might be to refer to the organic impurities contained in our streams the cause of many ailments, yet, I think it would be unfair to refer to these, and to these alone, the cause of an outbreak which carried off 37,000 in the short space of seven months. The fact that such large numbers were attacked at the same time pointed to a highly vitiated state of the atmosphere, and more especially to a peculiar condition of the soil.

In January, 1867, a paper by Dr. Salisbury of New York fell into my hands on the causes of intermittent and remittent fevers. The etiology of malarious diseases which that distinguished man has discussed with such talent and perseverance, led me to prosecute similar researches in the

town of Port Louis, and though I cannot positively state that I have been able to confirm all his views or repeat all his experiments, I can safely assert that the microscope revealed to me the existence of an immense variety of zoosporoid cells, animalcular bodies, algoid cells, filaments and fungoid spores in the damp exhalations, and vapours rising from stagnant pools, swamps, and humid low grounds about the town.

The examination of the secretions of the individuals attacked with intermittent fever, however, failed to show me the cells and the peculiar palmella which Dr. Salisbury found in the urine and expectoration of his patients, and to which he ascribes the cause, and at the same time the contagiousness, of the fever. I must candidly confess, however, that my hurried and no doubt imperfect observations throw no light on the importance of the facts adduced by the American professor on the existence of algoid plants in the emanations of swampy or low grounds. That such is the fact my observations leave not the least shadow of doubt; that these plants will eventually be found to be the primary cause of the mischief of intermittent fever, further researches may ultimately prove. As the last accounts from Mauritius shew that the fever had not lost its grasp on the population, a wide field still remains open for further observation and study.

To sum up: if we believe that excessive temperature can engender disease, we find that cause fully exemplified in the intense heat of January, February, or March of last year in Mauritius. If we believe that the influence of winds coming from quarters where the malarious generation was very rife can cause intensity of the fever, we find that again brought into evidence by the westerly and north-westerly breezes which prevailed so much and for so long a time during the same months. If we believe that a fever originating probably in a marshy locality, can assume other characters by its importation into another country, through telluric, atmospheric, or hygienic conditions, we find that fact illustrated by the Bombay fever in Mauritius. If we believe that intermittent fever is due to the poisonous effluvia arising from water acting on vegetable and animal matter, we find that cause clearly indicated in the condition of the soil of Port Louis. If we believe that intermittent fever is not amenable to any other drug than quinine for its speedy termination, we have abundant proofs that part of the excessive mortality in the epidemic of

Mauritius was due to the want of that invaluable medicine in the early part of 1867.

Such were the facts, Gentlemen, I wished to bring before you. It is not my province to discuss what remedial measures are necessary to restore to Mauritius its pristine splendour or far-famed healthiness. Projects entailing enormous expenditure for drainage on a grand scale have been brought forward as the only means to attain that object. From a consideration of the facts which I have been unfolding before you, you will see that our coral reefs if not carefully attended to will, if the scheme is carried out, cause a regurgitation into the interior of the town of enormous quantities of sewage, and that the proposed cure may eventually lead to other evils more permanent and still more disastrous in their effect.

NOTES ON
THE EPIDEMIC OF MALARIAL FEVER
WHICH APPEARED IN MAURITIUS IN 1866-1867.

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[*Read December 2nd*, 1867.]

The year 1867 will long be remembered in Mauritius as one of the most disastrous in the annals of its history. An epidemic of unprecedented virulence and intensity broke out with the beginning of the year, and in the space of a few months its passage was marked by upwards of 30,000 victims.*

Business suspended, commerce at a stand still, public offices all but empty, railway traffic stopped, anxiety and distress in every household, such were the principal features which to the outer world characterised the presence of the epidemic amongst us. A total want of quinine for nearly three months and absence of even the ordinary Peruvian bark, added considerably to the difficulties encountered in combating the disease, which by its rapid and wide spread progress, not only paralysed all the branches of industry, but brought on so thorough a state of misery and destitution that thousands of those attacked died from sheer starvation.

The disease has been called in England "yellow fever." As so erroneous a designation might lead to the inference that the epidemic was one of genuine West Indian yellow fever, it is but right that such a false impression should be at once removed. The fever was purely malarial or paludal in origin and character and may with propriety be termed "malignant or pernicious intermittent fever." It was similar in nature and effects to the disease so common at certain seasons in Madagascar, so fatal in the generality of cases and such an object of dread to all classes of Europeans there.

The icteric tint, which may have given rise to the appel-

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* The official statement of the mortality from all causes brought down to the 31st of July, 1867, gives 35,657 as the exact number for the whole island.

lation yellow fever, was only seen in cases of the bilious remittent form. In the present communication I propose considering what may have been the origin and cause of the epidemic, the mode of its propagation, the symptoms presented by the disease, and the means adopted to combat the pestilence. Before doing so, however, it will be necessary for me to take a retrospective glance at the sanitary condition of the island for some time previous to the present outbreak.

For several years past Mauritius had been suffering from a succession of droughts which, due to an extensive and indiscriminate clearance of forest land, in certain districts had seriously compromised the agricultural prospects throughout parts of the island. As in consequence of this extensive denudation, the annual amount of rainfall had decreased, the climate was rendered drier, and the soil became more arid and less fit for cultivation. As the land in the lower latitudes became less productive, the soil in the higher and colder regions, was soon extensively cleared of wood and called into cultivation. As a result, the rivers and water-courses began to dry up, rains became much less frequent but at the same time more violent, and an amount of rainfall which, in previous years, would have been beneficial, in 1865 brought on an inundation. I may also mention that for several years past thunder and lightning have been of much less frequent occurrence, and since January, 1863, hurricanes have been unknown. For months after the inundation of 1863, fever of a low or typhoid type prevailed throughout the island, but more especially in and around Port Louis. In January 1866, the first case of malarial fever broke out in the district of Black River, (west southwest side), in a European gentleman, the proprietor of the Gros Cailloux estate, and on the tenth of the following month, the men employed on the estate were struck down in rapid succession with intermittent fever; from that date the fever spread very rapidly throughout the whole district, attacking both Europeans and natives alike, and seemed to mitigate in severity about May or June when the cool weather set in. The month of December 1865 had been marked by sixteen days incessant rain followed by a scorching heat and intense dryness.

It is a remarkable fact that the outbreak of the fever at the Gros Cailloux, occurred immediately after the clearing out of a "Barrachois" (or arm of the sea where the salt and fresh waters meet)—the mud of which, about twenty

feet deep in certain places was thrown on the sides of the gully, exposed to the rays of a tropical sun, and when half dry carted into the cane-fields. The barrachois was generally covered over with water, but in consequence of the intense dryness, the bank became exposed to the sun's rays; and noxious exhalations as a consequence were given off.

Here, then, I think is one of the starting points in the development and propagation of the disease in this colony; and in the face of this I do not understand how the opinion has gained ground in certain quarters,—viz. that the disease has been imported,—can possibly be maintained. It is true that a batch of cases of malarial fever (intermittent) characterised by enlarged spleen and cachexia, were admitted into the Civil Hospital in 1864, from a vessel which had then just arrived from the Mutlah (a river close to Calcutta) where, as is well known, intermittent fever is endemic; but I am not aware that a single patient then in the wards caught the disease from the new comers. Malarial fever of the bilious remittent form, has been for many years endemic in Mauritius, but it has never given rise to other forms of fever, and pernicious intermittent has hitherto been a disease unknown.

Among the predisposing causes, then, of the disease, may be enumerated, the great droughts which in successive years have occurred throughout the island, the indiscriminate clearance of forest land, the gradually diminished amount of rainfall, and the inundation of 1865. If to these causes, we add, the intense heat of the past summer, the insalubrious state of the town of Port Louis, devoid of all system of drainage, the utter absence of rain for six months previous to the dawn of the present year, the agglomeration in Port Louis of a population which had increased out of all proportion to the size of the town, the filthy state of the dwellings of the lower classes, where all sanitary rules are utterly disregarded, the wholesale poisoning of the air by carts containing night soil, which perambulate the streets by day, the effluvia arising from stinking gutters, the stagnant condition of the streams and rivulets, in and around Port Louis, the emanations from which are not only most offensive to the smell, but positively injurious to health, the bad state of rivers and canals throughout the island, abounding in decaying vegetable matter, the impurity of the water generally, the objectionable practice of washing in flowing streams soiled linen, often contaminated with the discharges of the sick and the dying, I think we shall find causes sufficient to account

for the breaking out of the pestilence. In addition to these, other causes were silently at work, not so active perhaps, yet contributing their share to the propagation of the disease; these were the stirring up of the soil (which for ages past had been lying dormant) to make room for a line of railways, and the intersection of the town of Port Louis by trenches for the laying down of the gas pipes. These causes act more powerfully, I apprehend, than is generally supposed in the generation and diffusion of malaria. The same thing was witnessed in Hong Kong several years back, where fatal malarial fever broke out when extensive excavations were made for building purposes, and a severe epidemic of ague broke out in Paris a few years ago, during the construction of certain boulevards.

When we reflect that any one of the causes above-mentioned may give rise to a disease of a malignant character, is it to be wondered at that a combination of all, excited by an intensely hot and dry summer, should have given rise to one of the severest and longest epidemics on record?

When the fever broke out in and around Port Louis, its ravages were confined to the low lying districts of Salines and Cassis, which skirt the harbour, abound in marshy lands, are adjacent to an overcrowded cemetery, and border on a low foreshore, from the exposed sides of which putrid emanations due to decomposing animal and vegetable matter, are constantly given off. In addition to this, certain parts of the district are used as a receptacle for the filth and refuse of the town. It was in this quarter that the malaria appeared more concentrated, was more tenacious, and acted with more severity on the human frame.

I know not whether it was mere coincidence, but I several times remarked, especially when the wind was in from the sea (north-west, or Madagascar wind, as it is called in Mauritius), that a bluish mist, such as was described by Glaisher during the prevalence of the last cholera in England, overhung the low moist localities above mentioned. It was during these times that the disease was most severe, that relapses were more common, and the mortality greatest. As the weather became cooler, and as the wind veered to the opposite or north-east side, the mist disappeared, and the severity of the disease became mitigated.

A peculiar feature in the fever I have attempted to describe, was the suddenness of its invasion—persons in the fulness of health and strength were struck down as if by lightning—in some cases, the premonitory symptoms were

either of very short duration or altogether absent; in others, the invasion of the disease was marked by languor, lassitude, loss of appetite, frontal headache, severe muscular pains, horrible dreams with want of proper sleep by night, nausea, and vomiting often incessant and difficult to control. The matters vomited were at first the contents of the stomach, then thin frothy fluid, and finally bilious matter, but I never noticed the dark bottle-green dejections or the coffee ground matters so frequently thrown up in case of bilious remittent fever. As the disease became more severe, there came on intense pain about the præcordia, with difficulty of respiration and excessive action of the heart; a feeling of rush of blood to the head, and impaired or double vision. In the generality of cases the invasion of the disease was very rapid; a person after a hearty meal would, perhaps, be seized with intense rigors, and shake fearfully for one or two hours. The sensation of cold was so great, that no amount of extra clothing or blankets could keep the patients warm; the sensation of cold along the cerebro-spinal axis was compared by the sufferer to his being immersed in a tub of ice.

During the first or cold stage, or period of concentration of the poison, the lips, face, and nails became quite blue; the temperature of the body was, if anything, very slightly diminished—certainly not below one deg.; the urine free and clear, was intensely acid; there was sometimes ardor urinæ, or even deficient secretion; pulse feeble; skin pale and wrinkled; frequent drowsiness and headache.

The second or hot stage, or period of reaction, which usually lasted five or six hours, was characterised by intense heat of skin, flushed face, fulness of pulse, eyes suffused and glistening, and intense headache. The tongue generally furred, the urine high coloured.

To this succeeded the third, or sweating stage, or period of elimination, during which the perspiration was profuse and intensely acid. I have seen cases where the bed on which the patient lay was literally saturated. During this stage the urine became loaded with lateritious deposit. I have never, however, in the cases of intermittent fever, seen the bloody urine, sometimes amounting to genuine hæmaturia, so frequently met with in cases of bilious remittent fever.

After the profuse perspiration, then came a period of intermission, during which there was complete apyrexia, and the patient free from pain, but left in a literally helpless and prostrate condition.

During the course of the disease, especially the second stage, there was even marked pain about the liver and spleen.

Periodicity was a characteristic feature of the disease. One paroxysm, unless checked by appropriate treatment, was sure to be followed by another in twelve, twenty-four, or forty-eight hours; and in the more chronic cases the septenary period was principally observed. In women, I have noticed the disease to recur at the monthly times, when a slight show of blood manifested itself, or even when the catamenia were suppressed. The evacuations mostly liquid and bilious, became in the more chronic cases of a blackish colour, and very offensive. The quotidian type was most frequently observed, especially at the outset of the epidemic; and in these cases the disease appeared more amenable to treatment. It was generally the type of fever in those who contracted the disease for the first time. The tertian was most rare, and of the quartan I have only seen a few cases. The time at which the paroxysms occurred varied greatly, but in the generality of cases it was between ten A.M. and four P.M.

When the disease was simple and uncomplicated, it yielded to a few doses of quinine; but when it assumed a serious aspect, the period of intermission became very short, the different phases merged into one another, and then the febrile affections assumed a pseudo-continued form.

The question of contagion is one which, to my mind, cannot possibly be admitted. If any proof were necessary in support of the fact, it would be found in the immunity which our hospital attendants enjoyed for a period of two months after the breaking out of the epidemic, although harassed with fatigue, and knocked up by the fearful overwork. Again, of the many hundreds who fled from Port Louis during the intensity of the fever, to the healthy districts of Grand Port and Savanne, separated from town by extensive ranges of mountains and vast belts of forests, not one to my knowledge, succeeded in communicating the disease to the inhabitants of the districts. Nor do I know of a single instance in which the fever was communicated in former years by a person arriving from Madagascar, where, as is well known, intermittent fever is endemic, to a person residing in Mauritius.

A strange fact I have observed in connection with this fever, is the impress of periodicity imparted to other diseases during the prevalence of the epidemic. This was well exemplified in my own person during an attack of facial neuralgia, when the paroxysms of pain came on regularly about 9 A.M. The toxic influence of the disease was manifested in the number of abortions which took place during

the intensity of the fever; pregnant women far advanced would barely reach the ninth month; in many labour came on about the sixth or seventh, and most of the children were either born dead or presented symptoms of malarial poisoning: enlarged spleen and confirmed cachexia. If the child survived, it was only for a few days, or at the utmost for a few weeks. Labours were long and tedious, in those who had been the subject of fever; and the uterine inertia frequently necessitated the employment of the forceps. Post partum hæmorrhage was as a matter of course very frequent.

At the outset of the disease, menorrhagia was of usual occurrence, and it was always a subject of remark from patients, that they should again be troubled with the "monthly flow" where it had perhaps ceased shortly before. The hæmorrhage in these cases was profuse, and lasted during the whole course of the disease. In children and in adults of delicate constitution, the febrile affection was not unfrequently attended with violent epistaxis, which in several cases to my knowledge proved fatal. In women and those mostly of the middle age or advanced in years, the fever was not uncommonly ushered in by a violent attack of urticaria, during which the itching was most intolerable and distressing and lasted for several days; in others, a sensation of numbness and formication was experienced in the upper and lower extremities. In many cases the patients complained of feeling paralysed; of late, in many cases of relapse, I have been assured by patients, that an inordinate appetite—one perfectly monstrous in fact—was the precursor of the disease.

The recurrence of fever in women frequently took place at the time when the catamenia should appear; in a few, the menstrual flow would just come on, and after an hour or two cease, the blood in every instance being thin and watery. During the distressing vomiting which occurred at the outset of the disease, I have seen an enormous quantity of worms thrown up not only by children, but by adults of every age, class, and sex.

The pathological changes presented by those who died, were principally confined to the spleen, heart, and liver. The spleen varied in size, consistence, and colour—firm and succulent in those who succumbed at an early period of the epidemic; soft, pulpy, or rotten in the older cases, or when the pernicious form had existed for some time. The size varied considerably; in some the hypertrophy was hardly marked, in others the organ extended from the left hypochon-

drium to the crest of the ilium, and from the left kidney to the border of the rectus muscle. In old cases the substance of the spleen was quite creamy and of a chocolate colour, while in recent ones, the usual purple colour existed. As a rule, however, the viscus was greatly enlarged. In two or three instances I have met with rupture of the organ, the laceration generally extending from the hilus upwards, but in none have I met with signs of suppuration of the parenchyma. To my mind, the changes induced in the spleen are the more characteristic of intermittent fever.

Next in succession to the spleen, I think the changes presented by the heart most important. This organ was, as a rule, soft and flabby, with one or two white patches on its convex surface towards the apex. The right side was generally filled with a softish discoloured clot of an amber colour, sometimes the clot was blackish and quite soft. The left cavity contained either very little fluid, blackish blood, or was quite empty. There was no staining of the valves either with blood or bile, as is so frequently seen in cases of bilious remittent fever. When the disease had existed for some time, the organ was found either atrophied or considerably enlarged—in cases of hypertrophy—the right ventricle was usually dilated, and the left side much thickened.

The blood-vessels usually contained thin liquid blood, like a mixture of blood and water, which, under the microscope, showed an abundance of white cells and considerable diminution in the red globules.

The lesions presented by the liver were the following:—Intense congestion of the organ, with dark blackish blood exuding on section; enlarged in size in those who died during the earlier periods of the disease; contracted and hard, and of a slate colour, in older cases; in some, contracted, puckered, and granular, especially in cases of old standing, where the hepatic tissue appeared contracted and the interlobular hepatic substance filled with fat. The gall-bladder contained thick dirty ochre-coloured bile, and in no instance showing the black tarry, sluggishly flowing bile peculiar to the bilious remittent forms of the disease; and in no case did I ever meet with inflammation of the walls of the gall-bladder or bile-ducts. In all the *post mortem* examinations I have made, I have never found suppuration of the liver in intermittent fever. The changes presented by the brain were never commensurate with the severity of the symptoms during life; beyond congestion of the membranes of the brain, and numerous vascular dots visible on section, together with some amount of fluid in the ventricles, there

was nothing to note at the *post mortem* examination of those who died from a recent attack of pernicious intermittent. The changes induced in the organ in cases of old standing will be recorded further on.

The lungs, as a rule, were healthy in texture, but gorged with blood at their posterior portions. The bronchi were generally injected and filled with frothy serum. With the exception of a singular vascular zone at the lower border and œsophageal end, there were no traces of inflammation about the stomach. The duodenum was generally healthy, or, at most, presented slight traces of congestion in the mucous and submucous tissues. In many cases, the hepatic and biliary ducts were blocked up with lumbrici. Beyond the duodenum, the whole intestinal track was generally healthy, and contained some fluid fæcal matter. The kidneys, as a rule, very congested, with traces of fatty degeneration of the cortical substance in the Indians, and in many cases in the Creole Blacks, but not in the Europeans; the convex surface generally mottled and dark; the suprarenal capsules invariably healthy; the urinary bladder mostly distended with dark-coloured fluid.

With regard to the complications which arise during the progress of a case of pernicious intermittent, and to which the disease owed its fatal character, it is requisite that I should enter at some length. I know of no case, however, of simple uncomplicated intermittent which terminated fatally; when death occurred, it was either in consequence of rapid relapses, or of a single prolonged attack of malignant fever, the fatal result being due either to central mischief or utter prostration of the nervous system. Among the complications, in some cases the stress of the disease fell on the brain and cerebro-spinal axis, and these were characterised by drowsiness, persistent delirium, ending in coma, or in convulsions, mostly of an epileptiform character. The delirium varied in many cases: it was excessive loquacity in some; muttering in others; maniacal delirium at times passing into confirmed mania.

A second group of cases were marked by intense prostration of the physical and vital forces; here the algide form predominated, which was certainly the more frequently fatal of all complications. The patient complained of burning all over, while to the touch the skin was very cold; the very breath was of an icy coldness; the colour of the skin was livid; the fingers and nails quite blue; the features pinched up; the secretion of urine arrested; it was the algidity of cholera. In these cases, the intellectual faculties remained

clear to the end. The pulmonary complications arose from pneumonia and bronchitis, and were principally observed during the change of seasons. Diarrhœa, dysentery, gastro-duodenal irritation, and slight jaundice made up the additional complications.

The morbid appearances shown in those who died in a comatose condition were usually opacity of the arachnoid, with infiltration of fluid in the subarachnoid space; large patches of puriform lymph overlying the serous membrane of the brain; injection of the pia mater; effusion of serum into the ventricles, and a large quantity of fluid at the base. In cases characterised by epileptiform seizures, there was a thick layer of extravasated blood overlying the cerebral surface, with depression of the convolutions; in a case of old standing there was an hæmorrhagic clot in the anterior lobe of the left hemisphere, in the midst of disorganised soft creamy surrounding cerebral substance. The cerebral lesions found in those who died during violent delirium were: intense congestion of the brain and its membranes; sinuses gorged with blood; numerous vascular dots on section, and lateral ventricles filled with fluid. In cases of algidity, the softening of the spleen and heart was specially well marked. The liver was soft and flabby; the membranes of the brain infiltrated with serosity, and the lacteal ventricles filled with fluid.

Among the intermittent affections I have noticed during the progress of the disease in its pernicious form, may be enumerated:—

1. Sloughing of the integuments of the scalp.
2. Sloughing of the penis.
3. Frequent and prolonged epistaxis.
4. Sloughs over the trochanter major, and bed-sores.
5. Abscess of the scrotum.
6. Abscess of the jaw, with necrosis of maxillary bone.
7. Acute parotitis, with suppuration.
8. Gangrenous stomatitis, and sloughing of a large portion of the buccal mucous membrane.
9. Purulent collections above the shoulder and ankle-joints.
10. Scorbutus and purpura hæmorrhagica, accompanied with profuse uterine hæmorrhage in young, delicate females.
11. Menorrhagia independent of parturition.
12. Acute ophthalmia.

Among the sequelæ may be enumerated—

1. Peeling of the skin on the palms of the hands and soles of the feet.

2. Pharyngeal paralysis, and regurgitation of liquids through the nostrils, in one or two cases I have noticed in children.

3. Enlargement of the tonsils, and of the whole glandular apparatus.

4. Amenorrhœa, which persists for several months.

5. Boils.

6. Intense rheumatic pains across the joints.

7. Prurigo in elderly persons; and even ecthymatous eruption, in children and adults, producing a most violent and distressing itching.

The patches of ecthyma are invariably present after long and repeated attacks of fever. The affection shows itself at first in very small vesicles, the size of a pin's head, filled with a clear pellucid fluid, which becomes opaque, and finally filled with pus. The favourite seat is on the hands and feet, clefts of fingers and toes, on the anterior and inner surface of the thighs and legs, and over the glutæal region. Among the Indians and Creoles, and those whose system is considerably depressed, the vesicles merge into one another and cover the whole body.

Among the nervous affections observed, as a sequela of the disease, may be noticed:—

1. Dimness of sight and double vision.

2. Dimness of hearing.

3. Impaired memory.

4. Confirmed mania and melancholia.

5. Intense headaches, which remain for a long time after the manifestation of the disease.

6. Complete insomnia, with hallucinations at times.

7. Facial neuralgia.

8. Deep-seated pains across the bones.

Among the sequelæ, however, none is more characteristic of malarial poisoning, than the anæmia so commonly the result either of repeated attacks, or of a single prolonged pernicious intermittent; it seems to be the most powerful manifestation of the disease, and the point to which all the different forms converge.

The cachectic condition so commonly met with at present, both in hospital and private practice, is marked by intense weakness, by a discolouration of all the tissues and of the skin, by anasarca, dropsy of the peritonæum, irregular and excited action of the heart, by a bulging of the walls of the

stomach, and by difficult and painful digestion. A person affected with cachexia suffers from complete prostration of all the physical and vital forces—the power of the intellect becomes obscure, there is utter inability to exertion, lowness of spirits, difficulty of breathing, the symptoms of which mostly resemble those of asthma; there is great lassitude, loss of appetite, continual headache, deep-seated muscular pains. The skin, generally dry, is at first of a dirty yellow or saffron colour, and resembles when the disease is more advanced the colour of old wax; the whole body of the patient appears transparent, the eye is dull and lacks lustre, the conjunctiva of a pearly white, the cellular tissue about the eyelids becomes infiltrated with fluid, the face is puffy, the penis distorted, the scrotum œdematous. The pulse is generally small and soft, and beats 120 pulsations per minute; the carotids throb forcibly, and an anæmic bruit is heard when the stethoscope is applied along their course; the tongue is large, pale, and glassy, with serrated edges; the urine is scanty, frequently clear, and devoid of albumen, at times smoky, and affected by heat and nitric acid. The bowels are irregular, costive occasionally, but more frequently loose; the evacuations bilious and watery. With all this there is usually a slight febrile exacerbation at night.

The spleen is mostly enlarged, and reaches several inches below the false ribs; the liver is not perceptibly developed, and not painful.

Death in these cases comes on through syncope, colliquative diarrhœa, coma, or through some pulmonary affection. A *post mortem* examination in these cases reveals œdema about both lungs, frequently collapse of the lower lobes. At times the lungs are bloodless, and give exit to a thin serous fluid on section. The heart, bathed in a small quantity of yellowish serum, is soft and flabby; atrophied at times, much enlarged at others. The abdomen is full of a turbid serous fluid, the coils of the intestines, which appear macerated, are matted together by puriform lymph. In two instances I noticed a milky serous fluid, similar to chylous urine, in the abdomen and pericardium. The liver is, as a rule, small, indurated with contracted tissue, and the interlobular hepatic tissue filled with gas. The gall bladder is generally empty, or contains at the utmost a small quantity of dull ochrey bile. The walls of the stomach are thinned, and present numerous sago-like elevations when the mucous tissue is scraped. The same appearances are observed in the duodenum. I have never met with the ulcers which exist in cases

of bilious remittent fevers. The whole intestinal mass appears macerated from prolonged imbibition of fluid. The spleen is soft and flabby, with thickened capsule, on section quite pulpy, and of a dull chocolate colour. The kidneys are commonly small and atrophied, with traces of fatty degeneration of the cortical substance; at other times the organs are large and white, with fatty degeneration on section. In one case alone did I meet with purulent deposits in the substance of the kidney.

The lesions presented by the brain are remarkable: the dura mater and membranes are often quite bloodless, the cerebral substance blanched and softish, the convolutions frequently depressed, the lateral ventricles filled with pale serum; in other instances there is slight opacity of the arachnoids with infiltration of fluid in the subarachnoid space, the base of the brain being full of fluid. In several instances, when death took place from coma, there have been large flakes of puriform lymph overlying the opacity of the arachnoid, and in two cases I have met with several ounces of pus, between the dura mater and cerebral mass. In patients who had epileptiform convulsions during life, I found a thick layer of extravasated blood overlying the right side of the brain. In one instance I have met with pulpy softening of the cerebral substance surrounding a small clot of blood.

Treatment.—This was for a time rendered nugatory by a want of quinine and Peruvian bark. Arsenic was then extensively prescribed, but it could not be given indiscriminately to all. When quinine was procurable, it certainly proved the remedy *par excellence;* but when its market value was £28 an ounce, few could benefit from its use.

Of all the antiperiodics which the epidemic produced, quinine and the cinchona hold the first place. The action of the quinine was sure, prompt, and decisive, and when not given in enormous doses, it was the best and safest remedy. On the circulatory system its action was marked by slowness of the heart and pulse, and in large doses I have seen it produce coma. I have never myself given more than twenty or thirty grains a day, and have found the dose quite sufficient.

On the cerebro-spinal system it seemed to exert a sedative action; in cases of pernicious fever, accompanied with epileptiform seizures, if given during a moment of intermission, its action was remarkable. In chronic cases I have frequently observed gastritis supervening on an excessive and prolonged use of the drug. I have observed also that when

the salt could not be tolerated by the stomach, it was followed by the best results when administered by the bowels.

In chronic cases, quinine appeared less powerful, and seemed to exert a less beneficial effect than bark; its action seemed to be transient, while that of the cinchona was more permanent. When quinine has failed in my hands, especially when treating old cases of relapse, I have used cinchona with the happiest effect.

Next in importance must be ranked arsenic, which I consider a most valuable febrifuge, which has stood us in good stead in the hour of danger. I have given arsenic in large and repeated doses, and have never met with any evil effect from its use. During the intensity of the epidemic I have invariably given thirty drops of Fowler's solution in mucilage every two hours up to the fourth or fifth dose, the last dose anticipating the expected paroxysm by at least two hours. Its use was, of course, discontinued as soon as symptoms of gastric irritation—a silvery state of the tongue, with red elevated papillæ at the tip, and lachrymation—indicated that the drug could not be further tolerated. In a few instances I have employed cafeine, but its action as a febrifuge was uncertain, while the disorders it produced in the vascular and nervous systems rendered its general use undesirable.

It is hardly necessary to state that, here as elsewhere, numberless reputed specifics were recommended during the progress of the disease; among them I may mention:—Hydrochlorate of ammonia, sulphur, common salt, coffee, champao (Magnolia Champaca), chiretta, cadoc (Guilandina Bonduo), the clerodendron heterohyllum (Herbe or Bois Cabri), etc.

Most of these were excellent tonics, but certainly did not possess any antiperiodic virtues. With the exception of the cadoc, I have used them all, and found them wanting in febrifuge qualities.

Emetics and purgatives at the outset of the disease, followed by ten grains of quinine in the period of apyrexia, constituted the treatment I usually adopted; the quinine was repeated about a couple of hours before the expected paroxysms, and continued for one or two days. In the comatose state which so frequently followed attacks of pernicious intermittent, ice to the head, a purgative injection, and a dozen leeches behind the ears when necessary, were the means I usually adopted. I never resorted to general bloodletting, which I consider dangerous in the extreme. During the slight period of intermission, which followed on attacks

of pernicious intermittent, I generally gave ten grains or fifteen grains of quinine by the mouth, or, where that was not possible, twenty grains per rectum.

The intense headache, dimness of vision, and photophobia, so frequent and so distressing in the severer cases, were met by small blisters to the temples, and doses of valerianate of zinc, camphor, and belladonna. The tolerance of belladonna in these cases, as well as in cases of delirium and mania, was very remarkable.

In the treatment of the more chronic cases when associated with anæmia and painful digestion, I have found nux vomica, with some bitter infusion, of excellent effect. In the weak and cachectic, a wine-glassful of tincture of cinchona (℥j—Oj) every morning, and a teaspoonful of the syrup of the phosphate of iron, quinine, and strychnine, twice daily after meals, have been followed by excellent results. At the same time, when possible, I invariably recommend cold bathing, which I consider an admirable prophylactic.

But frequently, in spite of all treatment, the disease progresses, and then our only hope is in a change of climate, which, to be beneficial, must be resorted to before serious organic disease has set in.

Conclusions.—From the foregoing observations, we may, I think, fairly conclude:—

1. That the epidemic which prevailed in Mauritius in 1867, was purely malarial or paludal in origin, nature, and effects.

2. That the disease was of an infectious character, not imported, but of local growth, and fostered by want of proper sanitary measures.

3. That the cause of the malaria was due, in the first place, to the indiscriminate and extensive denudation of land, which has been going on for several years past; and, secondly, to a succession of droughts consequent on a marked decrease in the annual rainfall.

4. That among the secondary causes may be enumerated the inundation of 1865, the enormous increase in the population of Port Louis, the filthy state of the dwellings of the lower orders, the want of a proper system of drainage in the town of Port Louis, the absence of hurricanes for several years past, and the great diminution in thunder and lightning.

5. That the disease was one which was utterly unknown in Mauritius previous to the present epidemic, and that the

affection known as bilious remittent fever, though endemic in the colony for many years past, could not have given rise to an outbreak of malignant or pernicious intermittent fever.

6. That the disease is likely to become endemic unless prompt sanitary measures be adopted, and sanitary regulations enforced. Among these, the replanting of trees on an extensive scale should be made compulsory.

7. That a proper system of drainage should be immediately put into execution.

8. That the pestilence is one over which suitable hygienic measures can exert a powerful control.

9. That in the treatment of the disease quinine was found the specific *par excellence*, and that when it failed in its curative action, death was caused by complications affecting the brain and nervous system, or was brought about by sheer prostration of all physical and vital force.

10. That of all the other so-called specifics none were deserving of the name except it be arsenic.

FURTHER OBSERVATIONS ON THE INFLUENCE OF PANDEMIC WAVES IN THE PRODUCTION OF FEVERS AND CHOLERA.

By ROBERT LAWSON,

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[*Read 2nd March*, 1868.]

About ten years ago, while I was in charge of the Medical Department of the Army in Jamaica, I commenced an examination into the records of mortality at the various military stations in that island, to ascertain whether they indicated the operation of local causes that might be removed. I had proceeded but a little way before it became apparent that a cause of far more extensive operation was concerned, and, that until its laws were detected, there was no probability of any satisfactory deduction being drawn from the history of disease at the respective stations. I accordingly endeavoured to trace the course of epidemics, but, after a long consideration of all the evidence within my reach, was unable to fix on any law, though the fact that the epidemic influence was often experienced over a large extent of the earth's surface at the same time was clearly made out.

In the end of 1861 I was induced to examine into the course of fever in Jamaica again, and had scarcely begun it when the former difficulty presented itself, and the inquiry was immediately turned in that direction. After much consideration I was led to the conclusion that epidemics proceeded regularly from south to north, and that they might be traced from the Cape of Good Hope to Iceland. They succeeded each other at intervals of a few years, and often affected places far distant in longitude, and their resemblance to a succession of waves led to their causes being designated Pandemic Waves.

As the course of these waves seemed uninfluenced by atmospheric currents, it was clear they must be owing to some terrestrial force, and their appearance at Mauritius

[illegible]

[illegible] appeared in the [illegible] Reports on the health of the army for [illegible]. A [illegible] on the progress of cholera during the present century is ready for publication, and will most likely appear in the forthcoming volume of the Army Sanitary Reports.

Since my return to England I found there was a map of the Isodinic Magnetic Lines by General Sabine for 1840 in Johnston's *Physical Atlas*, and I have altered the original chart in conformity with this. A copy of the new chart on a large scale has been prepared for the Epidemiological Society, and it has been suggested that a notice of the principles on which it is formed, and their application to the elucidation of epidemics, would constitute a fitting accompaniment. In what follows I will endeavour to convey the leading principles as shortly as possible, and illustrate them by a few examples. Those who are anxious for fuller details are referred to the original papers.

I.—PERIOD OF THE WAVES.

If the records of disease at any place when fever constitutes a considerable portion of the sickness be examined, it will be found that, for the most part, the serious outbreaks occur either in the odd, or in the even years, according to its geographical position. There may be epidemics in the alternate years, but far more frequently they show themselves in the year peculiar to the station. Thus at Sierra Leone there were some cases of yellow fever in 1817, a severe epidemic in 1823, another in 1829, one case in 1835, a severe epidemic in 1837, three cases in 1845, another severe epidemic in 1847, and another again in 1850 all of

them odd years. In Jamaica there were very severe epidemics of yellow fever in 1819, 1825, 1827, 1831, 1837, 1841, 1853; there was considerable mortality in the even years, occasionally, from fever, but, as will be shown hereafter, the excess fell on the odd years. Yellow fever appeared at Gibraltar in 1804, 1810, 1813, 1814, 1828, besides threatening to do so on several other occasions; of the five periods four are even years, and one only an odd year. These facts show the waves to have a period of two years, or of some multiple of that, but on the whole the two-yearly period seems to represent them best.

The relative influence of the first and second years of the wave is brought out very prominently by the mortality among the troops in Jamaica from 1817 to 1836. In abstract XXX of the Appendix to the *Statistical Report on the Health of the Troops in the West Indies* for these years, the deaths from all causes, in the regiments of the line in Jamaica, are given for each month of every year. There are no means for separating the mortality from fever from that arising from other causes; but as the deaths from specified diseases amounted to 121·3 per 1,000 of mean strength, while those from fever alone constituted 101·9 of this ratio, or five-sixths of the whole, and it is well known that the excess of mortality in all the epidemics arose from fever, the fluctuations of the death rate may be taken to represent pretty accurately the influence of the Pandemic cause, acting of course through local causes and personal susceptibility.

Arranging the deaths for the twenty years by months, they are lowest from March to June inclusive, and highest in November; and if those for the twelve months commencing with March of the odd years be placed together, and those commencing with March of the even years similarly treated, and their sums taken, the aggregate mortality each month during the first and second years of the wave will be obtained. These numbers are as follow:—

Period of Wave.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	Total.
1st year	145	123	99	124	236	569	498	474	568	474	332	229	3871
2nd year	135	149	149	171	162	188	170	165	250	320	228	159	2246

These figures if projected in a curve bring out clearly the much greater activity of the causes of fever under the first

year of the wave than under the second; but it is obvious from the latter that a considerable epidemic is even then possible, though on a series of years the excess of mortality occurs under the first year. The form of the curve for August and September in the first year, as compared with its position up to July, and subsequent to October, indicates a disturbing cause, which on examination was found to arise from the excess of mortality in those months in the 50th and 92nd Regiments (just arrived from England) in 1819, and in the 84th (also recently from home) in 1827. There was a similar disturbing cause in the months from March to June of the second year, with the 33rd and 91st Regiments in 1822 (mostly from fever likewise), and with the 77th in 1825. Subtracting the deaths in these corps for twelve months after their arrival from the aggregate monthly mortality already given, the following numbers are obtained:—

Period of Wave.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	Total.
1st year	97	94	95	120	177	265	359	419	515	397	297	210	3045
2nd year	110	93	94	136	157	184	168	153	239	311	198	151	1994

These results show a regular progression from the minimum in April to the maximum in November the first year, and a less symmetrical one from the minimum in April to the maximum in December of the second.

There are no materials in the Statistical Reports on the health of the army to permit of the progress of fever from month to month being traced for other stations as for Jamaica, but the deaths from fever being given for each year, these have been reduced to their millesimal ratios to the strength, and those for the odd years, and those for the even added together, and divided by the numbers of these respectively, so as to obtain mean annual results. The statistics for the Home Station embrace seventeen years, for Gibraltar twenty-nine years, the other Mediterranean and the American stations thirty years, Ceylon, Jamaica, and West Indies twenty years, Mauritius, and Cape Town nineteen years, and Cape Frontier thirteen years only. These stations with their mean annual mortality from fever per 1,000 of strength, taking them as they come under the influence of the advancing wave, are—

Stations.	Millesimal ratio of mortality from fevers of all descriptions. In odd years.	In even years.
Mauritius	0·99	2·28
Cape Frontier	0·28	2·10
Cape Town	2·22	1·68
Ceylon	16·27	30·49
West Indies	36·63	36·25
Jamaica	124·63	82·65
Ionian Islands	12·07	8 81
Malta	1·73	3·33
Gibraltar	1·52	10·96
Bermuda	26·81	3·31
Home Station, Dragoons ...	1·63	1·18
„ Foot Guards	2·54	1·96
„ Infantry ...	2·69	2·77
Nova Scotia	1·83	1·07
Canada	2·51	2·09

The ratios for the even year at Gibraltar, and for the odd at Bermuda, are materially increased by the mortality in 1828 at the former, and by the epidemics of 1819 and 1843 at the latter; but if for the excessive death rate in these years, that of the highest in another even or odd year, respectively, be substituted, the mean annual ratio for the even year at Gibraltar comes out 3·26 as against 1·52 for the odd year, and at Bermuda 5·31 for the odd year as against 3·31 for the even one, still showing the excess as in the first instance.

The period when fever appears at the different stations is an important consideration in this inquiry. At Mauritius, the Cape, and in the southern hemisphere generally, the local causes seldom present any marked activity before the beginning of December; fever seems to be most frequent in February and March, and then to decline to July, or even to extend into August. In Ceylon, from September to March, there is little mortality from the disease; it attains its greatest force in April, and declines through the following months to August. The course of mortality in Jamaica is obvious from the details already given; and that of the Windward and Leeward Islands, or the West Indies as they are denominated in the military returns, does not seem to be materially different. In the Mediterranean and American stations, and Bermuda, fever commences to increase in June; it seems to culminate under ordinary circumstances in August, and then to decline. At Gibraltar, however, though cases of yellow fever have been met with as early as the middle of July, it was not before the end of August that the disease spread, and it attained its greatest height towards the end of October.

II.—PROGRESS OF THE WAVES.

As mentioned above, the waves move from south to north. They take five years to go from the Cape of Good Hope to England. Any period of the year might be fixed on to lay down their position. I have selected the 1st of January as the most convenient and best suited to the statistics which were available. The lines on the chart are lines of equal magnetic dip or isoclinals, and were selected to represent the position of the advancing edge of the wave from year to year. Those on the chart are 70°, 53°, and 30° south; and 0°, 30°, 53°, 70°, and 80° north. These represent the progress of epidemics with unexpected accuracy, but it is quite possible that, as the records of disease assume greater precision at different places, it may be found necessary to substitute others more or less distant from them.

A febrific wave is supposed to start from the isoclinal, 70° S., on the 1st of January of a year with an odd number, and to overspread the zone between that and 53° in the course of the year. It will start from 53° on the 1st of January of the even year, and so on. As the waves are assumed to recur every second year, another will leave the isoclinal, 70°, on the next odd year, and will pursue the same course as that which preceded it. From this peculiarity it follows that the first part of a wave is experienced in any given zone, either always in an odd year, or always in an even one, and from this simple arrangement results that the wave on the 1st of January in the

	Odd years.	Even years.
Will leave the isoclinal ...	70° S.	53° S.
"	30° S.	0° S.
"	30° N.	53° N.
"	70° N.	80° N.

And the force of the epidemics is accordingly felt in each of these zones in the year peculiar to it, following that of the zone to the south of it, and preceding that of the one to the north.

There is an apparent exception to this in the case of Mauritius, the Cape, Ceylon, and Bermuda. The first three lie to the north of zones which are overspread by the wave in an odd year, and can be reached by it only towards the end of the year, while fever prevails in all in the early months, hence the excess of mortality occurs in these in the even year, the first occasion on which the local causes acquire their activity after the arrival of the wave. Bermuda, near the northern limit of a zone overspread by the wave in an

even year, has its severest epidemics in the odd year following.

As the progress of the wave from year to year is regular, so its advance through any given zone seems proportional to the period of the year which has elapsed, and the position of the margin may be thus shown approximately. Applying these principles to the mortality at the different military stations given above, it is found that at Mauritius, and on the Cape frontier, the excess falls on the even year, in accordance with the theory. Cape Town is an apparent exception; this arose altogether from the high mortality in 1825 and 1827, when numerous deaths took place from typhoid fever, presenting the usual intestinal lesions, and which was distinctly traced to the causes now recognised to originate them, and on the removal of them, the disease, which was confined to certain spots, ceased. Ceylon agrees with the theory also. In the next zone, Jamaica affords the strongest support to it, but the West India Islands give an undecided result, the cause for which I have not been able to detect. As Sierra Leone in the same zone on the east of the Atlantic, however, agrees with Jamaica, there is every reason to conclude that there must have been something in connection with the West Indies which masked its development. In the zone beyond the isoclinal, 53° N., the Ionian Islands present a similar anomaly to the West Indies, while Malta and Gibraltar both give unequivocal support to the theory. Bermuda in this zone, as already stated, has the severest epidemics in the odd year, though they are not confined to that. Beyond the isoclinal, 70° N., the cavalry and foot guards, which at that period may be considered as having had almost their whole service at home, have their excess of deaths from fever in the odd year. The infantry, which from 1830 to 1836 included the men of the West Indian depôts, mostly recruits, for the next ten years embraced the whole of the regiments on home-service, all of which had recently returned from abroad; this fact may have had something to do with the different result obtained for this portion of the force from those for the cavalry and foot guards. The ratios for Canada and Nova Scotia both show their predominance of mortality in the odd year, according to the theory.

III.—THE INFLUENCE OF THE WAVES IN EXCITING EPIDEMICS OF FEVER.

The influence of pandemic waves in causing outbreaks of fever is manifest in two ways, viz.: firstly, under the operation

of the same wave, in the same year, at points often very different in latitude and embracing in longitude almost half the circumference of the earth; and secondly, in successive years, at points more and more to the northward, as the waves pursue their usual course. To illustrate these fully it is necessary to examine the records of disease at a sufficient number of distinct and widely spread stations for a series of years, when it will be found that many epidemics, which have hitherto been thought independent of each other, were local manifestations under the same general influence, and others which were considered accidental, and to account for which much labour has been expended and ingenuity displayed, fall into their place naturally as subordinate to the general law. A few such illustrations may be given here.

A wave is supposed to have advanced from the isoclinal, 53° S., on the commencement of 1816, but there are no data available for the Cape or Mauritius that year or the following one. The first indication of its operation is met with in in Ceylon 1818, when the mortality among the troops from fever, chiefly of the remittent form, rose to 133.8 per 1,000 of strength. At Ascension, where fevers occur in the same year as at Ceylon, the admissions in 1818 were twelve, six times more numerous than in 1817, and embracing nearly every man on the island; one death occurred, the symptoms, according to Burnett, being those of yellow fever. At Lima, in Peru, a large body of Spanish troops from Europe was assembled, among whom a severe fever, supposed to have been yellow, broke out in February 1818. The disease was not confined to Lima, but was met with as far as Ica, in latitude 14° S. In 1819, this wave, extending from Ceylon to Peru, would overspread the zone beyond the isoclinal, 30° N. This year Pali plague was very severe in Cutch and Guzerat. The plague was epidemic at Susa, on the south of the Mediterranean. The mortality in the West Indies was less than in 1818 or 1820, but in several of the subordinate stations it was high, and at Demerara, one of these, where the form of fever for years previously had been the ordinary remittent, yellow fever appeared and proved very fatal. At Jamaica the mortality among the troops rose to 273·8 per 1,000. In 1820 when the wave would proceed beyond the isoclinal 53° N., the mortality in the Ionian Islands was low. At Malta it was more considerable for that place, and at Gibraltar it increased, while in the neighbouring parts of Andalusia yellow fever prevailed epidemically, and typhus was severe in the convict hulks at Toulon.

At New Orleans where there had been a considerable mortality from yellow fever in 1819, it remained nearly as high in 1820. In 1821 the wave would proceed beyond the isoclinal 70° N. There was little fever in Nova Scotia this year, but in Canada the ratio of deaths was 2·8 as against 0·6 the previous year. At Bermuda the ratio rose to 14·3 from 3·0 in 1820, and at Norfolk in Virginia, and Baltimore, yellow fever showed itself. A fever denominated catarrhal prevailed in Iceland in 1821.

Another wave would advance from the isoclinal 53° S. on the 1st January, 1822. At Cape Town the mortality per 1000 this year was 2·3 as against 0·0 the previous year, and on the Cape Frontier it amounted to 4·8. There was no death from fever at Mauritius in 1822. In 1824 this wave would advance from the isoclinal 0°, and in Ceylon the deaths amongst the troops rose to 104·3 per 1000 from 20·0 the preceding year. The first Burmese war commenced this year, and the troops engaged suffered very severely from fever and dysentery, and that form of fever called Dengue appeared among them. The Ashantee war was also going on in 1824, in the vicinity of Cape Coast Castle on the West Coast of Africa; here there was excessive mortality among the Europeans employed in the field, from fever and dysentery. In 1825 the wave would enter the next zone; Dengue spread from Calcutta along the Ganges a considerable distance. Plague was very severe in Egypt. This year a body of white troops was sent to Sierre Leone and the Gambia, among which the mortality from remittent fever was excessive. There was a high mortality in the West Indies, but rather less than the previous year, but in Jamaica the ratio per 1000 rose to 287.8, chiefly from yellow fever. The wave would pass into the zone beyond 53° N. in 1826. Plague was prevalent at Constantinople this year; the ratio of mortality among the troops in the Ionian Islands rose from 8·3 the previous year to 11·0. At Malta there was no increase; at Gibraltar it rose from 3·2 in 1825 to 4·2, and Hennen reports there were several cases of yellow fever. There was a high mortality from yellow fever among the troops in the Bahamas. In 1827 when the wave would enter the next zone, the mortality in Bermuda rose to 4·5 from 1·6 the previous year; in Nova Scotia it rose to 2·7 from 0·7, and in Canada to 4·6 from 3·2 in 1826. Typhus prevailed in Iceland in 1828, extending into the following year.

In 1845 the sickness broke out in the *Eclair* on the Coast of Africa, which has since caused so much controversy. One

argument strongly insisted on by those who thought that the epidemic, which appeared at Bona Vista that year, arose from communication with the cases labouring under yellow fever, was that there was no evidence of an epidemic constitution being in operation, and hence its aid could not be invoked to account for the prevalence of the disease after she left. By reference to the chart it will be seen that, like Sierra Leone, that island lies in the zone between the isoclinals 30° and 53° N., and the advancing edge of the wave would reach it about 18th August of the odd year. The *Eclair* left Bona Vista on 13th September, and the epidemic did not commence among the inhabitants who had had no communication with her or her people until the 12th October, the usual sickly season there, and it was preceded by heavy rain. It may be doubted by some when the cases on board the *Eclair* first assumed the character of yellow fever. I quite agree with the compiler of the Reports on the Health of the Navy, that some of those which appeared when she was at anchor off the Seabar, to the south of Sierra Leone, those of the stoker on the 23rd May, and of Field on 4th June, were not to be distinguished from those that occurred subsequently, in short, were really cases of yellow fever. There is no doubt, too, that at least three cases of this disease proved fatal on shore at Sierra Leone, altogether independent of the *Eclair*, two of the subjects having not even arrived in the colony at the time she sailed. There is thus evidence that in the year when fevers were to be expected in that zone, yellow fever appeared in the *Eclair*, and on shore at Sierra Leone independent of each other, and subsequently, but not before, the wave had reached it, at Bona Vista. So far these may be considered by some as coincidences merely, but on tracing the course of this wave it is found that at Santa Cruz in Teneriffe, in the next zone, an epidemic prevailed in 1846, resembling, if not actually, yellow fever, and the millesimal ratio of mortality among the troops from fever rose in the Ionian Islands from 2·5 in 1845 to 6·8 in 1846; in Malta from 1·1 to 3·0; at Gibraltar from 0·9 to 6·7; and at the last named station one case presented yellowness of conjunctiva and skin, and so much resembled yellow fever that an epidemic was anticipated. In 1847 this wave extended to the British Islands, leading to a great development of febrile disease. These facts leave no doubt that the independent manifestations in the *Eclair* and at Sierra Leone were connected with the pandemic influence, under which also Bona Vista came the same year, and which was so distinctly

manifested at Teneriffe and through the Mediterranean the following year, and in England the subsequent one.

The outbreak of yellow fever in Brazil in the end of 1849, took place under similar circumstances. Rio Janeiro, where it commenced on 28th December, is so situated in the zone between the isoclinals 30° S. and 0°, that the wave would reach it about the middle of October of the odd year, and the epidemic commenced, as is usual in the southern hemisphere, soon after the summer solstice there.

In 1853 a severe epidemic of fever occurred at Monte Video. Opinions were at variance as to whether it was yellow or not; but the possibility of yellow fever arising there was placed beyond doubt by its appearance in 1857. This place lies between the isoclinals 53° S. and 30° and the wave would reach it about 1st November of the even year, and as at Rio Janeiro fever was to be looked for at the commencement of the following, in this case the odd year. I have not the exact date of its appearance in 1853; but in 1857 it commenced in February and continued to the end of May. In continuation of the epidemic of 1853 at Monte Video the severe epidemic at Lima in the beginning of 1854 shows the progress of the same wave. This would pass the isoclinal 30° N. on the 1st January 1855, and in the course of that year there was an extensive epidemic of the febrile affection denominated "Matlazalhutl" in the southern portion of Mexico. Cholera was common in several islands in the West Indies in 1855, and in the end of the year there was a partial outbreak in Jamaica, and, as seems usual where this disease prevails, fever was little met with, but in 1856, the second year of the wave, there was a pretty severe epidemic of yellow fever in Jamaica and at Havanna. There was a slight epidemic in 1856 at Bermuda, and a few cases at Lisbon, Oporto, and Corunna, but here, as in the West Indies the previous year, cholera was rife in 1856, which seems to have checked the development of the fever, through the first year of the febrific wave at Lisbon, and it was only in 1857, after the disappearance of the cholera, that the yellow fever became epidemic there. This alternation of fever and cholera is frequently met with, the conditions suitable for the development of fever appearing adverse to cholera, and *vice versa*, those favouring the extension of cholera checking the appearance of fever.

IV.—THE CHOLERIFIC WAVE.

An examination of cholera epidemics brings out the fact,

that they, like those of fever, move with regularity from south to north, and that their local manifestations from year to year may be embraced by a succession of waves, which advance at the same rate, and are limited by the same lines of magnetic dip, as have been employed in connection with those of fever. There is this difference between them however, that, on a series of years, the maximum of cholera occurs in any given zone the same year with the minimum of fever there; in other words, that the first year of the cholerific wave coincides with the second of the febrific, and thus the cholerific wave on 1st January in the

	Odd years.	Even years.
Will leave the isoclinal ...	—	70° S.
"	53° S.	30° S.
"	0° S.	30° N.
"	53° N.	70° N.

There are few places, where cholera forms one of the prevalent diseases, for which the records of mortality are available in such a form as to permit of the monthly and annual death rate being defined, as has been done for fever in Jamaica in the former part of this paper; but a table of the deaths at Bombay from February, 1851, to January, 1864, inclusive, which appeared in a paper by Dr. Macpherson in the *Medical Times* of 23rd November last, gives the necessary details for that place. The sum of the deaths each month, for the fourteen years, is lowest in September, and as the cholerific wave is supposed to overspread the zone between the isoclinals 0° and 30° N. in the odd year, by taking the numbers for the twelve months commencing with September in that for the first year of the wave at Bombay, and similarly those commencing with September in the even one for the second year, their relation to each other is brought out as follows:—

Period of Wave.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Total.
1st year	368	646	972	1547	2244	1556	1813	2629	2327	2027	1153	738	18,020
2nd year	507	472	429	886	1052	1173	1457	1443	1457	1945	1159	601	12,581

If these figures be projected as a curve the inflexions are rendered quite distinct, and leave no doubt as to the existence of a wave in connection with cholera as with fever, and of the intensity of cholera coinciding with that portion of the febrific wave when its influence is least. The lowest point in the curve, too, agrees within narrow limits with the

interval between the receding and advancing waves, as deduced, either from the actual magnetic dip at Bombay for the mean period, or from actual measurement on the chart. The mean dip at the Bombay Observatory for 1857-58 was 19·2°. And as the wave is supposed to move from the isoclinal 0° to 30° N. in the course of the odd year, it should reach Bombay about the 20th August, while measurement in the chart gives the 6th August as the approximate date.

Cholera is met with in three forms, viz., the epidemic, the sporadic, and the common bilious or English cholera, and there is much diversity of opinion as to the relation these bear to each other. I have met with sporadic cases, within the tropics, which presented every symptom characteristic of the epidemic form of the disease, and many others have had similar experience. I, therefore, believe they were essentially of the same nature as those which when they occur in greater numbers constitute an epidemic. The common or bilious form, though resembling the epidemic in some respects so much as to receive the same name, still differs not only in the character of the intestinal evacuation, but in the absence of the suspension of the function of the kidneys so common in the latter, and, so far, they present much the same affinity to each other that intermittent does to yellow fever. Like these, too, when the epidemic cholera prevails, other places in the same zone, often very distant in longitude, frequently present an unusual number of the sporadic or bilious cases, though far beyond the limits of the epidemic disease, thus indicating that all these forms are influenced by the same general cause. A single illustration may be given.

In 1825 a wave passed to the north of the isoclinal 0°, and the mortality from cholera among the white troops in Ceylon rose to 21·4 per 1000 from 0·6 the previous year. In 1826 when this wave would overspread the zone beyond 30° N. the deaths among the white troops in Bengal rose to 21·1 per 1000 from 6·7 the preceding year; in Bombay the ratio fell a little, but was still high. Cholera was epidemic in China this year. In the West Indies the ratio of admissions rose from 9·0 in 1825, to 24.0 per 1000, though with but one death. In Jamaica there was one case only, which proved fatal however, and was the first death from this disease there for ten years. In 1827 the epidemic disease spread beyond the isoclinal 53° N. into Tartary. This year the ratio of admissions in the Ionian Islands, which had been high for several years, continued with little change. At Malta there was an increase, and at Gibraltar a diminu-

tion in the admissions. In 1827, too, the second year of the wave, the admissions in Jamaica increased to 16·5 with a death, and in the West Indies they fell to 9·3 with three deaths. In 1828 the cases at Bermuda were 45·6 per 1000, there having been none the previous year, and this year also at Gibraltar the admissions were nearly quadrupled. The admissions in Nova Scotia were 3·1 per 1000 in 1828, and in Canada 6·1, but as in Jamaica and Gibraltar in the second year of the wave here, 1829, the ratio rose to 11·2 and 15·4 respectively, with three deaths in Canada. There are numerous other instances of the same description.

According to the theory announced above, the first part of the cholerific wave would overspread the British Islands in the even year, and this is quite in accordance with the facts. The epidemic which commenced in the north of England in the end of 1831, was under the second year of the same wave which had reached Orenburg in August, 1829, and which in 1831 had also caused the disease in Iceland. The epidemic, which commenced in the south of England and Canada in 1832, was under the first year of the following wave. The epidemic of 1834 was experienced at Havanna, and Lisbon and south of Spain, in 1833, and reached England and Canada the following year. The epidemic, which passed northwards through to Russia in 1847, was felt beyond the isoclinal 70° N. in 1848, and became apparent in England late in the year only, as the fever then prevalent declined, but in 1849 it became fully developed. The occurrences of 1853-54 were parallel to those of 1831-32, the first appearance of the disease in the north of England in the former year was under the second year of the wave which had led to the outbreak at St. Petersburg and in northern Russia in 1852, and the epidemic of 1854 was experienced in the south of Europe in 1853. The same explanation applies to the epidemic of 1866 which may here be examined somewhat more in detail.

The first indications of this wave I have met with were at Natal in South Africa, and Mauritius, in 1861. At Mauritzburg in the former, in the end of January or beginning of February, diarrhœa with watery evacuations was prevalent, and one case occurred presenting all the characters of epidemic cholera, with complete collapse. This information I had from Dr. Mann who attended the patient, and who had been familiar with cholera in England. There were six attacks among the troops in Mauritius in the end of 1861, four of which proved fatal, and in 1862 a severe epidemic ensued. This wave would pass the isoclinal 0° on 1st January, 1863,

and this year there was a slight increase in the attacks and mortality among the troops in Ceylon, and the ratio of mortality of the previous year was continued among those in the Madras Presidency. There was also a fatal case among the troops in St. Helena. The wave would reach Bombay in the course of August this year. The deaths from cholera there, from September 1862 to August 1863, the second year of the previous wave, and those for the twelve months from September 1863, the first year of that under consideration, were as follow:—

Period of Wave.	Sept.	Oct.	Nov.	Dec.	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Total.
2nd year	161	272	201	269	189	50	89	161	153	161	412	240	2358
1st year	178	181	176	319	622	401	302	680	837	395	371	351	4813

These figures bring out most distinctly the influence of the advancing wave in increasing the mortality over that of the previous year, and this is obvious in every month, except October and July. The mortality among the white troops in the Bombay Presidency was 2·1 per 1,000 only in 1864, and was confined to the Bombay, Poonah, and Mhow divisions, but among the native population in their vicinity the disease was much more active. In Bengal there was little cholera among the white troops, and it was chiefly met with at Lucknow and Allahabad. In 1865 the deaths in Bombay itself were slightly less numerous than in 1864, but the details for each month are not given, which prevents a full comparison with those under the first year of the wave there; these were, however, 431 in December 1864, and 363 in January 1865, indicating much activity of the causes of the disease at that time. Among the white troops in the Presidency the mortality rose to 15·9 per 1,000, cholera having appeared in all the military divisions, though to the greatest extent in the Bombay, Mhow, and Northern Divisions, and in the Bengal army the Saugor, Gwalior, and Oude divisions were visited severely. There was, then, an unusual activity of the causes of cholera in the tract of country extending from Bombay north-eastwards to the base of the Himalayas, while there were lesser degrees of the same met with at Poonah on the south-east, and at Hydrabad and Kurachee to the north-west. It was evidently in connection with these occurrences that cholera appeared early in 1865 at Makallah on the Arabian coast, and at

Hodeida in the Red Sea south of Jedda, and subsequently at Mecca among the pilgrims congregated there. All these places, like Hindostan, were under the second year of the wave, and the outbreak of the disease in the former, contemporaneously with its great aggravation in a portion of the latter, indicates the operation of a general cause quite distinct from those personal or local causes, to which alone it has been attributed. The reason for this cause not having embraced Arabia in its influence in 1864 as well as Bombay, cannot be given with certainty, but there is satisfactory evidence that in that year fever was very prevalent on the west coast of Africa between the isoclinals 0° and 30° N., that it prevailed along the course of the Nile, and especially at Khartoom, and that it was more among the troops, both in the Madras Presidency and in Birmah, than in 1863. It seems probable that that part of Arabia between the same isoclinals was similarly affected, and, indeed, the reports at the time were, that, up to the early months of 1865, low fever was very prevalent at Mecca. It has already been mentioned that the conditions which favour febrile epidemics seem adverse to those of cholera, and this, so far as our information extends at present, seems the most reasonable explanation of Arabia and Egypt having escaped cholera in 1864.

The wave under consideration, according to their regular progress, would pass the isoclinal 53° N. on 1st January, 1865. I am not aware of any evidence as to cholera having manifested itself in Tartary this year, but, as all are aware, after showing itself at Alexandria in June, it soon appeared at many points on the north coast of the Mediterranean, and began to penetrate the various countries where it showed itself, but now, unlike the rapid progress it had made between the isoclinals 30° and 53° N., its advance was gradual and regular, as it seems always to be under the first year of the wave, and at no place in Europe did it commence as an epidemic earlier than the wave would reach it, and though three were small isolated outbreaks near to or beyond the isoclinal 70° N., in the end of 1865, yet the disease did not become epidemic up to, or north of that until 1866, the year when previous experience indicated this might be expected. In America it observed the same law.

The length this communication has reached renders it unadvisable to add farther details; those desirous of these may find them in the original papers to which allusion has already been made.

REPORT ON THE RECENT EPIDEMIC OF CHOLERA (1865-1866).

By J. NETTEN RADCLIFFE.

[*Read April 6th*, 1868.]

In September, 1865, the Council of the Society presented to the Foreign Office a memorial concerning the then recent extension of cholera along the shores of the Mediterranean. After referring to previous pandemics of this destructive disease, the memorial stated that, "hitherto the information before the profession respecting the exact course of the development of cholera in the several epidemics which had traversed Europe, had been far from being either so correct or so complete as the interests of science demand." The desirability of a fuller and more accurate knowledge of the geographical history of the disease as it appears in different lands, and under the varying conditions of climate, country, the habits and diets of peoples and other conditions, was pointed out. It was remarked that epidemiological inquiries have to be conducted in much the same way as meteorological inquiries; and that to be fruitful of good, both must equally rest on accurate data collected in a wide area of observation, and over periods of time more or less extended. Further, it was said that "what the Board of Trade now does to advance the progress of meteorological science may be done by other departments of the Government to advance the interests of epidemiology. On no point," the memorial continued, "is authentic information more needed than as to the exact dates when the pestilence first appears in the different localities attacked over the face of the continent. Great Britain possesses greater facilities in obtaining such information, from her wide spread relations and intercourse, than most other countries. It is respectfully submitted that the object in view might be efficiently attained, through the co-operation of H.M. Consuls, if the Secretary of State for Foreign Affairs would be pleased to give instructions to those gentlemen to record and communicate the most reliable information within their reach in respect of their consulates and the surrounding districts, and if all such information were made available for the

benefit of medical science. To the following points the Council of the Society would particularly invite attention—

1. The exact dates of the earliest recognised or ascertained cases of the disease, whether the cases proved fatal or not.

2. Did these cases occur among strangers or persons recently arrived in the place?—or among residents who had not been recently away from it?

3. Had there been any unusual amount of bowel disorders, or other form of sickness, prevalent among the inhabitants prior to the occurrence of these cases?

4. In what part of the town or village did the first cases occur?—and what part or district suffered most during the visitation?

5. What is the nearest place where the disease was known to exist at the time of the occurrence of the first cases, or to have existed shortly before such occurrence?

6. What precautionary measures have been taken by the authorities to avert, or to meet, the visitation?"

In answer to this memorial, the Secretary of State for Foreign Affairs, Earl Russell, replied:—"That the various reports received from Her Majesty's Consuls relative to the outbreak and progress of cholera had been made over to the Lords of Her Majesty's Most Honourable Privy Council, by whom they had been handed to the Medical Officer of the Privy Council: and that their Lordships then intimated that this gentleman would permit the Secretary of the Society to refer to the reports or to any correspondence on the subject which he might have in custody."

After the receipt of this reply I waited upon Mr. Simon. He very courteously gave me every facility for consulting the reports and papers relating to cholera forwarded to his office; and I take this opportunity of expressing my warmest thanks to him for the assistance he gave me in prosecuting the somewhat onerous task I had undertaken. The material placed at my disposal for studying the diffusion of the epidemic was by no means so complete as it would have been had the simple suggestions of the Council of the Society been adopted and acted upon by the Foreign Office. The questions would, I believe, have proved welcome and valuable guides to the consuls in making their reports. With few exceptions the consuls of infected districts took a deep interest in the disease, and their reports were executed with great care. It needed but a few suggestions to direct their observations to make the reports of the highest value. It is to be regretted that the Foreign Office did not see good

to adopt the recommendation of our Council; but failing this we owe it much gratitude for placing within our reach all the information it received concerning the progress of the epidemic.

Whilst working at the history and progress of the disease, as recorded in the official documents sent to the Medical Department of the Privy Council Office, I was called upon by the Privy Council, at the suggestion of Mr. Simon, to report to their Lordships on the same subject. In carrying out the instructions of the Privy Council I was permitted to consult documents bearing upon the subject received at the India, Colonial, War, and Registrar-General's Offices. The mass of matter gathered from these different sources, and given in detail in my report published in the Appendix to the Eighth Report of the Medical Officer of the Privy Council, would otherwise have been first laid before the Society. I trust that I shall have the forgiveness of the Society for sacrificing, as it were, its undoubtedly primary claim upon my labours in the interest of the Government. I have no regret, I must confess, for doing so; as thus I was enabled to devote an amount of time to the subject which otherwise would have been impracticable. Moreover, as reporter to the Privy Council, the data placed at my command were more extensive than would have been the case as Secretary of the Society. These considerations will, I hope, prove my justification to the Society, if a justification be needed.

The Report referred to carries the history of the epidemic to the close of the year 1865; and beyond this period, that history as yet can only be very partially written. Indeed, some little time must elapse before the history of the epidemic can be completed—that is, such a history as will prove of scientific value. Official and trustworthy reports of the dissemination of the epidemic in the different kingdoms and states of the continent of Europe and in North and South America are but slowly appearing. It is most difficult to obtain these reports without the intervention of Government, and when obtained, a command of languages and time will be required to digest them, the thought of which may almost drive the most hopeful and enthusiastic epidemiologist to despair.

In October, 1865, when the epidemic had spread with alarming rapidity along the coasts of the Mediterranean and Euxine, and was ravaging several parts of the continent, the French Government suggested that an International

Sanitary Conference should be held at Constantinople for the purpose of "investigating the primary causes of cholera, determining its principal points of departure, studying its characteristics and its march, and, lastly, proposing practical means for confining the disease and stifling it at its origin." The reasons for suggesting that the Conference should meet at Constantinople were thus stated in the circular of M. Drouyn de Lhuys:—Owing to their geographical position, the countries of the Levant are first attacked by the scourge. The Oriental Government, therefore, is particularly interested in the ameliorations it is desired to introduce for the general welfare in the organisation of the sanitary service, and we may reckon with confidence upon its co-operation in the measures of which its subjects will be the first to feel the beneficial effects. It must not be forgotten that it is owing to the persevering assistance of the Porte, to the successful improvements it has introduced into the administration of the public health, that the problem of the suppression of the plague has been happily settled. It is from Turkey, therefore, that the Conference will be able to find the most efficacious aid to its labours; it is with her support that it will collect the best elements of practical solution. These considerations, the value of which will no doubt be appreciated by the Cabinet of ——, appear to me naturally to point out the city of Constantinople as the seat of the Conference. Brought into more immediate contact in this capital of the Ottoman Empire with the provinces in which the epidemic has its chief centres, the representatives of the foreign Powers will gain valuable information from the Superior Council of Health working under the supreme direction of the Porte. These favourable conditions will render easier in that city than anywhere else the accomplishment of the important mission to whose success the Administration of the Sultan will consider it an honour to contribute." The proposition of the French Government was acceded to by the principal Powers of Europe; and in February, 1866, delegates of these Powers met at Constantinople. One of the labours of the Conference was the preparation of an historical sketch of the progress of the epidemic in 1865. Of the different Reports prepared by the Conference, this sketch chiefly concerns me in reference to the subject of my present Report. It supplies several important details wanting in the Report which I had previously prepared for the Privy Council.

The historical sketch of the Conference and my Report,

read together, furnish all the important authenticated data respecting the first and most active period of dissemination of the epidemic. But in addition to these, there are now accessible various valuable and elaborate Reports (some official) and monographs on the early outbreak of the disease in the chief ports of the Mediterranean. Of these Reports and monographs, I would mention especially :—

Surgeon A. Leith Adams and Assistant-Surgeon F. H. Welch, 22nd Regiment.—Report of the cholera epidemic of 1865 in the Maltese islands. *Deputy Inspector-General Rutherford, M.D.*—General Report upon the Cholera Epidemic in Gibraltar during the summer and autumn of 1865. *Deputy Inspector-General Lawson.* — Extract regarding the Outbreak of Cholera in the Ship *Renown,* on passage from Gibraltar to the Cape of Good Hope. *Deputy-Inspector General Barrow.*—Narrative of the Outbreak of Asiatic Cholera on board the Steamship *England.* (These reports are all published in the Statistical, Sanitary, and Medical Reports of the Army Medical Department for 1864. *Dr. Sutherland.*—Report on the epidemic of cholera at Gibraltar 1865 ; and Report on the Sanitary Condition of Malta and Gozo, with reference to Epidemic Cholera, 1865. (*Blue Books*). *Dr. Ghio.*—The Cholera in Malta and Gozo in 1865. (Official Report: Malta 1867). *Dr. V. Seux*; Le Choléra dans les Hopitaux Civils de Marseille pendant l'Epidémie de 1865. (Pâris 1866). *Drs. Laugier et Ollive :* Etudes sur le Choléra de Marseille en 1865. (Marseille 1865.) *Drs. Didiot and Gués.*—Rapports sur l'origine du Choléra à Marseille en 1865. (Marseille et Paris, 1866). *Dr. Menecier.* Historique de l'Epidémie de Choléra à Marseille, 1865. Paris, et Marseille, 1866). *Dr. V. Collardot.*—Le Choléra d'après les neuf épidémies qui ont régné à Alger, depuis 1835 jusqu'en 1865. (Paris 1867.) *Dr. Louis Mongeri.*—Etudes sur l'épidémie de Choléra qui a régné à Constantinople en 1865. (Constantinople, 1866.) *Professor Parkes.* —On the Outbreak of Cholera at Southampton in September and October 1865. *Mr. J. Simon and Mr. J. Netten Radcliffe.* On the outbreak of cholera at Theydon Bois, in Essex, October 1865. (Eighth Report of the Medical Officer of the Privy Council). *Mr. J. Simon.*—Remarks on Cholera in the Leeward Islands (p. 25). *Dr. Buchanan.*—On imported Cholera among migrating foreigners in the Mersey. *Professor Parkes*—On cholera in Southampton (Ninth Report of the Medical Official of the Privy Council.) Report of the Commission of Quarantine and Health Officer of the Port o

New York; January 1867. (Albany, 1867.)—Annual report of the New York Metropolitan Board of Health, 1866. (New York, 1867.)

I propose to avail myself of the different sources of information here noted, and lay succinctly before the Society the principal facts concerning the first period of diffusion of the epidemic. I shall limit myself to these facts alone, stating them in the briefest fashion consistent with clearness; but I hope at some subsequent period to submit to the Society a more complete and comprehensive account of the epidemic.

State of health of population attacked prior to the invasion of the epidemic.—The first question which enlists the attention is the state of health of the different populations infected by the epidemic immediately preceding its invasion. Information on this subject is clear and decisive. The epidemic was not preceded by any of those signs which have been presumed to indicate forthcoming pestilence. There was no augmentation of diarrhœal disease, or marked deterioration of the public health, such as was believed to precede the epidemics of cholera in 1832-33 and 1848-49. In the majority of cases the health of the populations attacked was excellent as compared with previous states, and there was no peculiar prevalence of zymotic diseases. It is true that in Egypt the outbreak of the epidemic coincided with a period of much privation, but like privation was not present elsewhere among the communities attacked. It is true, also that Dr. Sutherland has shown that during the six months preceding the outbreak in Malta the amount of diarrhœal disease was greater than was the case in the corresponding six months of the previous year. But when, applying this fact, he writes:—"There is evidence that a change presaging the advent of an epidemic, had been taking place for months in the public health of the islands;" he ignores the fact that no such change "presaged the advent" of the epidemic at Gibraltar, as he satisfactorily shows in his report on the outbreak in that town and garrison. The inference in reality is much wider than the fact from which it is deduced and is not by any means a legitimate deduction from that fact as it stands. It is true, moreover, that MM. Didiot and Gués have endeavoured to show that prior to the appearance of cholera in Alexandria, choleraic disease existed in Marseille and that the outbreak of the epidemic in the latter town was preceded by "une constitution médicale cholérique." But the facts they advance in support of their conclusions simply show that deaths from infantile cholera, which, as they

properly remark, is "à la verité, fréquente à Marseille," and from so-called "sporadic," cholera are apt to occur in Marseille during the months of May and June, as in perhaps every city and town of central and southern Europe. Certainly they state that the cases of these diseases which occurred in May and June were characterised by unusual severity, and this fact must be taken for what it is worth. Neither in Paris nor in London, where statistical and other materials for solving this question abound, was there any change in the public health, which warned observers of the coming storm. As to London I have examined the question at some length in my report to the Privy Council on the recent outbreak in the metropolis, and I would refer to that report.* The truth is that the epidemic took Europe by surprise, and so generally good was the public health that the first news of the appearance of cholera in Egypt excited no attention and scarcely any alarm. It was not until the disease had effected a lodgement in several parts of Europe that the notice of governments and of the public was fully aroused.

The Starting Point of the Epidemic.—The first positive fact in the history of the epidemic was the arrival of two British vessels, the *North Wind* and *Persia*, at Jedda, in March, 1865, from Singapore, freighted with pilgrims, chiefly Javanese, for Mecca. On the voyage, after touching at Makalla, a sea-port on the south coast of Arabia, cholera appeared on board and carried off 145 of the passengers and crew, before the ships reached their destination. Other ships from India arriving at Jedda in the course of March and April, and carrying pilgrims, also had deaths from cholera on board. The captains of the ships first-named asserted that the passengers contracted the disease at Makalla, where it was present when the ships anchored there. The existence of cholera at Makalla at this period has been denied; but the probability is in favour of the disease being present there at that time. Prior to the malady breaking out at Mecca it had appeared at Hodeida; and probably about the same time the disease had shown itself at Aden and elsewhere in Lahej. Cholera, indeed, seems to have been prevalent at more than one point of the southern and western coasts of Arabia before the time of the Mecca pilgrimage. It is not improbable, also, that the disease existed on some parts of the African coast, south of the straits of Bab-el-Mandeb.

* Ninth Report of the Medical Officer of The Privy Council (Appendix.

H.M.S. *Penguin* captured two slave dhows, one on the 15th April, 1865, the other on the 12th May, running north in the Gulf of Aden. While conveying the dhows to Aden a considerable number of the slaves died from choleraic disease. Two of the ship's company also died, one on the 17th April, the other on the 16th May; and in both cases "the symptoms were precisely similar to those of epidemic cholera." At the period of the occurrences here noted Bombay was barely recovering from the severest outbreak of cholera from which the island, and probably the Presidency, had suffered for many years. Once before only (1852) since the registration of deaths there, that is to say within the seventeen years preceding 1865, had the island suffered so severely from cholera. (See "Deaths in Bombay, August, 1864.") Now it is to be remembered that the principal trade of Makalla is with Bombay; and it is not improbable that late in 1864 or at the commencement of 1865, cholera was introduced from Bombay into Makalla (not necessarily carried by pilgrims), and transmitted to other towns of the Arabian coast as well as to the coast of Africa. It is no fanciful theory that these coasts have a traffic in diseases as well as goods: for Mr. Hewlett, the Medical Officer of Health for the City of Bombay, tells us in his report to the Municipality for 1867, that small-pox "is frequently imported into Bombay from persons arriving in native craft from the seaport towns in Arabia, Persia, and Africa."

The Irruption of the Epidemic.—Cholera smouldered in Jedda from the time of the arrival of the infected ships; and before the Mecca pilgrimage was accomplished it broke out in the Hedjaz with great violence. Towards the close of the pilgrimage, in fact, its ravages became so alarming, that as soon as the rites were completed the assemblage broke up in a panic, and the different caravans hastened their route homewards, the disease following their steps. The events at Mecca can only be stated broadly, but of the general facts there cannot be much doubt.

The Spread of the Epidemic.—On the route of the pilgrims journeying homeward by way of Jedda, the deaths were numerous, and undoubtedly from cholera.

On the 19th May the first ship carrying pilgrims from Jedda arrived at Suez. It was an English steam vessel crowded with 1,500 of the returning devotees. On the voyage several deaths had occurred from unknown causes, and the bodies had been cast into the sea. On the 21st May the captain of this ship and his wife were both attacked with cholera.

On the 22nd May a case of cholera was recognised at Damanhour, forty-six miles E.S.E. of Alexandria, and on the line of railway from Suez, among a body of pilgrims coming from the last-named port.

Between the 19th May and 1st June many thousands of pilgrims were landed at Suez. Those who had simply to traverse Egypt on their route homewards by way of Alexandria were, by an arrangement of the Egyptian Government, immediately forwarded by railway to the last-mentioned port; and of these such as could not at once be embarked upon the vessels provided to carry them to their further destination, or had not the means to secure their passage homewards, were encamped outside the city near the canal of Mahmoudieh. The remaining pilgrims landed at Suez journeyed from Suez, and the stations on the line of railway to Alexandria, to their various destinations in Egypt.

There is not any account of the state of health of the mass of pilgrims who were encamped at Alexandria, nor of the different groups who were dispersed throughout Egypt journeying homeward. On the 2nd June a case of cholera (the first recognised) occurred among the inhabitants of Alexandria who were in communication with the pilgrims. On the 5th June two other cases occurred under the same circumstances. From this date cases became numerous, but until the 12th June, according to Dr. Aubert Roche, chief physician to the Isthmus of Suez Canal Company, the cases were confined to that portion of the population which was brought into contact with the pilgrims. On the 11th June cholera was officially reported to be present in Alexandria, and between that date and the 14th foul bills of health were issued to vessels leaving the port. Between the 2nd June and 23rd July the epidemic spread over the whole of Egypt, and it is estimated that, in less than three months, 60,000 persons were destroyed by the disease.

On the 20th June a case of cholera, the first of the subsequent outbreak in the island, occurred in the Lazzaretto island, Valetta, Malta. On the 31st May a steam vessel entered the harbour of Valetta, from Alexandria, with upwards of 200 pilgrims from Mecca, on board. She was admitted to pratique. The next day she sailed for Tunis, leaving 61 pilgrims at Malta. "Between the 31st May and 14th June, fourteen steamers arrived here (Valetta) from Alexandria," Dr. Ghio reports, "having on board, besides their respective crews, upwards of 845 passengers, of whom 426 were hadjis. Nearly all these were landed. The greater

part of them, the same day, or the next, proceeded in the same vessels to their destination; but some of them remained here several days." On the 14th June the first news of cholera having broken out in Alexandria reached Valetta, and on the evening of the same day a seven days' quarantine was imposed upon vessels arriving from the former port. That evening a vessel reached Malta having eleven pilgrims on board, and the master reported that during the voyage a pilgrim had died from bowel-complaint. On the 20th *The Rhone*, a steam-vessel, arrived at Valetta from Alexandria, bringing 147 passengers for Malta. During the passage of the vessel a passenger and a fireman had died from cholera. On the 20th June (the day when the first case of cholera occurred in the Lazzaretto island) the number of persons detained in quarantine at the lazzaretto amounted to 288, including the persons in charge. The distance between the locality where the first case occurred and the nearest part of the lazzaretto to which passengers from Alexandria had access is, in a direct line, 662 feet.

On the 21st July cholera appeared in the island of Gozo, contiguous to Malta. A seaman from Malta reached the island on that day and took up his abode with his sisters. He was seized during the night but recovered. On the 24th four cases occurred. Two of the patients were sisters of the seaman; a third was a relation who had constantly visited the house during his illness, and the fourth was a person living in the same village.

On the 24th June the first case of cholera occurred in Smyrna. No account is given of the arrival of vessels from Alexandria prior to the establishment of quarantine on account of cholera. On the 23rd June a steam-vessel coming from Alexandria landed 293 passengers at the lazaret, one of whom died from cholera on the 29th June.

On the 28th June cholera appeared in Constantinople. That day a frigate, the *Moukbiri-surur*, entered the Golden Horn from Alexandria, and the same evening twelve of the crew were sent into the marine hospital suffering from choleraic diarrhœa. Two deaths from choleraic diarrhœa—or more probably cholera—had occurred among the crew during the voyage. On the 5th July, cholera broke out in the barracks through which the patients were carried to the hospital, and about the same time the disease appeared among the labourers of the arsenal living in the suburb outside, and shortly spread thence to the whole city. An instructive episode of the outbreak, seemingly well authenti-

cated, was this: On the 11th July a workman left the vicinity of the arsenal and visited the village of Yeni-Keni, several miles from Constantinople, on the west shore of the Bosphorus. The same evening he was seized with cholera while at a café. On the 12th July—the day following—several Mussulman labourers and Greek boatmen who visited the same café were seized with diarrhœa. This was the commencement of a disastrous outbreak in Yeni-Keni and the vicinity.

On the 1st July cholera appeared at Jaffa. A Greek vessel had arrived that day from Alexandria. The captain of the vessel died from cholera the evening of his arrival. The next day a Greek was seized with the disease in one of the crowded houses of the town, and died the day after. This was the commencement of a very serious outbreak, and the malady accompanying a fugitive population spread throughout the surrounding district.

On the 1st July also cholera appeared at Beyrout. From the 17th of the preceding month a great influx of persons into the town, flying from Alexandria, had begun; and among the incomers were upwards of 400 pilgrims sent at the expense of the Egyptian Government, besides other pilgrims who travelled this route by preference. These pilgrims came from Beyrout, from Damascus and the neighbourhood, and from the districts intervening between the two cities. Many of the fugitives from Alexandria also passed through Beyrout to villages and towns inland. Several deaths from cholera took place among the passengers at sea, and all were subjected to a detention in quarantine of from five to ten days. The epidemic smouldered in Beyrout from the 1st to the 13th July, and then spread actively for several weeks. The epidemic appeared in Damascus on the 1st August; and in Jerusalem, probably during the second or third week of that month. The Persian pilgrims who took the route by Beyrout, Aleppo, and Diarbekir on their return homewards, reached the vicinity of Aleppo during the second week of August. The sanitary state of the individuals composing this caravan was execrable, and a part of its baggage consisted of the corpses of fellow pilgrims who had died on the journey. The first case of cholera in Aleppo took place on the 15th August. The day following the caravan was permitted to enter the city. The same day two quickly fatal cases of cholera occurred, and from this time the epidemic rapidly grew, and in three months carried off 7,000 people out of a population of 90,000—the ordinary population of the city, 120,000, having been reduced one-fourth by flight. Cholera accompanied the

caravan of Persian pilgrims, which traversed Aleppo, along its whole route to the Euphrates, breaking out at Orfa, Diarbekir, Mosul, and other towns on the route.

On the 8th July cholera appeared on the west coast of the Mediterranean at Valencia; and at Ancona, on the west coast of the Adriatic. On that day a French merchant, who had journeyed from Alexandria, by way of Marseilles, to Valencia, was struck down with cholera in the last-named town and died the same day. Other inmates of the house in which he died were next attacked. This was the starting point of the epidemic in Spain.

On the same day the first case of cholera occurred among the population of Ancona, four days after the arrival of a steamer from Alexandria. On the 3rd of July, a woman who had arrived from Alexandria was detained at the lazzaretto, where she remained six days. She left the lazzaretto on the 9th, was taken ill on the 10th while travelling towards Pistoia, where she died on the following day with all the known symptoms of Asiatic cholera. This was the beginning of the outbreak in Italy.

On the 18th of July, cholera appeared at Gibraltar. The first case was a private of the 22nd regiment, and other cases quickly occurred in the same regiment. The wing of the regiment in which the disease first showed itself had sailed from Malta on the 6th of July, after cholera had broken out there, and landed at Gibraltar on the 10th of the month. On the 5th of August, seven cases were reported among the civil population of the town.

On the 23rd of July, the existence of cholera in Marseilles was officially recognised. The first clear case of the disease occurred on the 18th of June, the patient being a nurse having charge of the infant of a fireman of a steam-vessel. On the 11th of June, the mail-steamer *Stella* arrived at Marseilles from Alexandria, having among its passengers sixty-seven pilgrims on their journey to Algiers from Mecca. The pilgrims were landed, and encamped within the fortifications near the port, and remained there a short time. Other vessels followed in rapid succession from Alexandria, and landed many passengers. On the 23rd of July, the epidemic was fully declared in Marseilles. Nineteen cases took place from the date given to the 31st, inclusive. The history of each of these cases was carefully investigated by Dr. Ménécier; and of these fifteen had more or less intimate relations with the port.

On the 17th of September, cholera appeared at South-

ampton, the English port most constantly and directly in communication with Egypt. The circumstances attending this outbreak have been investigated by Professor Parkes, and I need only mention it briefly here.

I do not propose to do more than refer to the chief instances of primary dissemination of the epidemic. It would take up too much time to trace its development along the shores of the Euxine and Sea of Marmora, following the track of infected vessels from Constantinople; its ascent of the Danube, and penetration of Russia from the Black Sea provinces; and I may only briefly refer to the probable, if not certain, importation of the disease, in August, from Odessa to Altenburg, in West Saxony, and subsequent development of the epidemic there; to the importation of the malady by the ship Atalanta into New York harbour, and further importation by other ships in the spring of 1866; to the probable carriage of the disease from Marseilles to Guadeloupe* in October and certain carriage from the same town to Algiers.

I have followed the track of the epidemic to the north and west and from the east coast of Syria. Now I must show its course in another direction. Early in August, cholera broke out at Mascat and Bender-Abbas. On the 4th of September the disease appeared at Bassora, and successively broke out, along the course of the Euphrates (ascending that river), at Kourra, Suk-el-suk, Samava, Divamieh, Imam-Ali, and Kerbellah, whence it passed to Hillah and Bagdad, appearing at the latter city in September. Along this route the development of the epidemic coincided with the passage of the Persian and Central Asia pilgrims returning from Mecca by sea, ascending the Gulf of Persia and the river Euphrates. The epidemic which thus broke out on the banks of the Euphrates, in the course of the land caravan by way of Aleppo, and Darbakir, as well as of the pilgrims returning by sea, passed into Persia, and has not yet exhausted its ravages.

Again, we learn from the official correspondence on Abyssinia and Dr. Blanc's narrative, *The Story of the Captives*, that cholera broke out in Massowa in October, 1865, and that in the autumn of that year and spring and summer of the following year, the disease had ravaged some of the remoter parts of Abyssinia, on the route traversed by the Takruri pilgrims on their way to and from Mecca.

The time at my disposal is too short to do justice to the

* See Ninth Report of Medical Officer of Privy Council, p. 26.

facts which I have thus briefly set forth. I trust, however, that I have said sufficient to show the salient points in the early dissemination of the recent epidemic.

1. There can be no serious question, I imagine, of its starting-point, Mecca, at the time of the Mohammedan pilgrimage of 1865—as far, at least, as Europe is concerned.

2. It is equally unquestionable that the diffusion of the epidemic followed certain lines of traffic, and that its development among populations had a definite relation to these lines of traffic, and to the movements of pilgrims returning from Mecca.

3. In not a single instance did the disease first show itself on the coast of Europe, or elsewhere, within our present knowledge of the spread of the epidemic, without communication with a previously infected locality.

4. In some instances the earliest cases were imported; in others there was no manifest importation of the disease, but the incoming of persons from a locality where the malady was prevailing. The latter instances have not been so thoroughly investigated as they ought to have been, except in respect of Marseilles and Southampton, and to the summing up of Professor Parkes in reference to the last-named towns I would refer. The practical difficulty of dealing with the question of quarantine rests chiefly on the doubts arising from the instances referred to.

As compared with the diffusion of previous epidemics of cholera, the spread of the recent epidemic presents several remarkable differences:—

1. For the first time in the history of epidemic cholera, Europe was invaded by the disease from the south.

2. While the epidemic of 1829-32 occupied nearly fourteen months, from the time of its entrance into Europe, in traversing the continent, fifteen months in reaching Great Britain, and two years, less one month, in arriving on the North American coast; and whilst the epidemic of 1845-48, crossing the continent of Europe in about the same period of time as the previous diffusion, reached New Orleans nineteen months after the appearance of the disease on the eastern border of Europe; the recent epidemic, in less than five months from its appearance at Alexandria, and six from its outbreak at Mecca, had spread from the coasts of the Euxine to the western hemisphere. This unparalleled rapidity of progress finds no explanation in any peculiar virulence of the disease, but solely, so far as I can see, in the increased facilities and greater rapidity of traffic between different countries.

ON THE EPIDEMIC OF CHOLERA IN THE PUNJAUB, THE NORTH-WEST AND CENTRAL PROVINCES, BENGAL, 1867.

By ROBERT SUTHERLAND,

ASSISTANT-SURGEON, 5TH LANCERS.

Communicated by the Director-General of the Army Medical Department.

[*Read 1st June*, 1868.]

(ABSTRACT.)

Sirhind Division.—On the breaking up of the Hurdwar fair on or about the 15th of March, it was reported to the official authorities at Umballa that several cases of cholera had occurred amongst the pilgrims; and knowing well that they would enter the station, unless prevented, the military authorities had cordons of police organised at a village called Taiplei; but before this was sufficiently organised, pilgrims had entered cantonments, and cholera was the attendant result. On the 19th of April, a prostitute in the bazaar of the 94th regiment, within fifty yards of the "Grand Trunk Road," was seized with cholera and died. "It is unknown whether she was in communication with the pilgrims or not." On the following day, another case occurred in the same bazaar, and on the 21st a third case. On the 22nd, two Europeans were attacked with cholera: one was a soldier of the 111th regiment, from Sunawar, who had on the previous day crossed the track of the pilgrims; the other was a man of the 94th regiment, who had been in company with the man of the 111th regiment.

On the 24th, twenty cases of cholera were reported as having occurred in the bazaar of the 94th regiment during the previous night, upon which the authorities had the natives residing in that bazaar moved into camp.

On the afternoon of the 23rd of April, an officer of the 94th regiment and a soldier's wife were attacked with cholera; the surgeon of the regiment and the two assistant-surgeons were reported as suffering from "choleraic diarrhœa." The case of the former ultimately became cholera.

Under these circumstances, the authorities ordered the regiment to be moved into camp about three miles south-

west of cantonment. There was a halt in the progress of the disease for two days. On the 26th, a patient in hospital was attacked, and on the evening of that day the regiment was moved to another encamping ground about four miles south-west of cantonment. From the 29th of April until the 4th of May, the regiment had taken up fresh ground on two different occasions, and as cholera continued to recur, it was deemed advisable to break it up into detachments. This was carried into effect.

The women and children also were encamped about twelve miles from cantonment. The results attending the segregation of the regiment were highly satisfactory, excepting in the case of the head quarters' camp, where diarrhœa became very prevalent; and one case of cholera occurred on the 5th of May; but after this date there was no further recurrence of the disease in the head quarters' camp, and none whatever amongst the other detachments, during the period of their encampment. The regiment returned to barracks on the 20th of May.

From the 23rd of April until the 8th of May, cholera was very prevalent in the camp of the native followers; but as the disease had disappeared from amongst them shortly after that date, they were permitted to return to their houses in the bazaar, which had been thoroughly cleansed during their absence.

One case only occurred on the 2nd of May in the battery of Royal Horse Artillery. The soldiers had been on the preceding day fishing in a "tank" near the station. The disease had a fatal termination.

In the bazaar of the 21st Hussars, a case of cholera is reported as having occurred on the 4th, and another on the 6th of May. On the 9th, a woman was seized with cholera and died. On the 13th and 16th, there was a case on each day. On the 18th of May, four cases were reported as having occurred in the bazaar. On the 19th, a woman of the regiment was attacked and recovered. There was then a halt in the progress of the disease until the 28th of May, when two more cases occurred in the bazaar. After that date, the period of quiescence was of long duration, as there was no fresh outbreak of the disease in the regiment until the 22nd of September, when two soldiers were attacked. In the 3rd Native Infantry, several cases of cholera were reported as having occurred on the 3rd and 6th of June, and diarrhœa was reported as having been very prevalent among them, "coincident with a scarcity of water at that end of the station, owing to the drying up of the wells."

Hill Stations.—As explaining the outbreak of the disease at Subathoo and elsewhere, it will be well to mention that the pilgrims who had been diverted from the Grand Trunk Road passed through Kalka (situated at the foot of the hills) on their way home; and between the 18th and 28th of May many cases of cholera were reported as having occurred amongst them, and subsequently amongst the natives of that village.

About the 18th of April, also, many of the pilgrims passed through Kussowlee; and cases of cholera having occurred in the latter bazaar, all communication between Kalka and Kussowlee was stopped, and the result was highly satisfactory. On the 26th, the road from Kalka to Kussowlee was reopened, notwithstanding the earnest protestations of the medical officer in charge of the depôt to the contrary, as on that very day a case of choleraic diarrhœa had occurred in the bazaar.

On the 19th of July a soldier was attacked and died, and choleraic diarrhœa was reported as having been very prevalent in the bazaar.

At Subathoo, the first outbreak of the disease occurred on the 23rd of April, in a woman of the 90th regiment, and diarrhœa became very prevalent amongst the men of the regiment, and subsequently the regiment suffered from cholera.

Jullunder.—During the whole of the hot season, cholera is reported as having been prevalent in the Jullunder district, having appeared there after the pilgrims had passed through. It was not until the 3rd of September that the first outbreak of the disease occurred in the 82nd regiment; first in the medical subordinates' quarters, then in No. 12 barracks, and subsequently in No. 6.

Lahore Division.—Although a few cases of cholera were reported as having occurred amongst the native population at Anarkullu and in the city of Lahore, it was not until the 26th of May that the disease broke out in the 106th regiment.

The first case of cholera occurred in the Fort of Lahore about the 15th of April; and although the disease was reported as having been prevalent amongst the native population, it was not until the month of August that the malady appeared at Govindghur; a second case in September; and both terminated fatally. The two men who died had occupied the same "bastion," which has been reported as having been otherwise a fruitful source of sickness.

Ferozepore.—For some time previous to the outbreak of

cholera in the cantonments of Ferozepore, cases of cholera were reported as having occurred in the native villages around cantonments, and the medical officers state that the disease was introduced by the pilgrims from Hurdwar.

On the 1st of June, a case of cholera occurred in the 15th Royal Infantry. It was not until near the end of that month that the disease appeared in the 15th Fusiliers.

Peshawur Division.—The first outbreak of cholera amongst the European troops at Peshawur occurred on the 20th of May. On the previous day, a severe and prolonged dust-storm passed over the station, and on the 20th, there were four cases of cholera in separate buildings on the right flank of the artillery lines. "Vomiting and purging were not prominent symptoms, but collapse set in early."

Simultaneous with the outbreak of the disease in the Royal Artillery, the 42nd Highlanders were also attacked with cholera.

Gwalior Division.—The superintending staff surgeon-major at Morar states it as his opinion that the outbreak of cholera which occurred in the Gwalior Division was introduced by the pilgrims from the Batissa and Hurdwar fairs. It was not until the end of May that the first case of cholera occurred in the 103rd regiment. On the 11th and 12th of June, two boys of one family were attacked and died; and simultaneous with the outbreak in the 103rd regiment, the disease appeared in the village of Koomhurpoor, described as filthy in the extreme. "Heaps of refuse at the gates of the houses, and in the compounds human excrement of many days' collection, lay exposed. The wells (within three feet of the gates) into which cesspools soaked, and the drinking water impregnated with organic matter."

Meerut Division.—Before the outbreak of the disease occurred amongst the troops at Meerut, cholera was reported as being prevalent in the neighbouring villages. It was not until the 15th of August that the disease occurred in cantonments, when two men of the Royal Artillery were attacked and died. These men occupied separate but contiguous barracks.

Allahabad Division.—As early as the month of May, cholera was reported as having occurred in some of the villages on the Benares portion of the "Grand Trunk Road," taking in its course the native gaol on the north bank of the Jumna. Cases subsequently appeared at Calpee and Cawnpore. It was not, however, until the 28th of July that cholera appeared amongst the troops in Allahabad, and at a time when cholera did not exist in the neighbouring villages.

ON ENDEMIC AND EPIDEMIC INFLUENCES IN PERU.

By ARCHIBALD SMITH, M.D.

[*Read 6th July*, 1868.]

The coast of Peru, from 3 deg. 30 min. to 21 deg. 30 min. S. lat., is naturally a continuous, arid, and sandy desert, which is here and there intersected by rivers, or considerable streams of water, that descend through rocky clefts from the Andes. By these channels, which are partially dried up in the dry season on the mountains or Sierra, the maritime valleys are partly irrigated and fertilised. In olden times the land of the Incas was extensively watered, through the medium of a most laborious and widely spread system of aqueducts, leading from the hills to the coast, which was closely peopled, as numberless ruins of large towns and adoratories bear witness. The most northern province of this coast range is called Piura, of which Payta in lat. 5 deg. 5 min. 30 sec. S., long. 81 deg. 8 min. 30 sec. W. is the well known seaport. Twenty-eight leagues inland from Payta, is the headland of this coast district, and it is occupied by three large estates, namely, Yupatera, Moropon, and Mono de las Padres. The aspect of this spot is indescribably beautiful. The surrounding hills are clothed in a lovely verdure of flowery luxuriance, and ample fields produce arrow-root, yucas, Indian corn, sugar cane, and all sorts of savoury and refreshing fruit matured under a uniformly clear and serene sky. When it rains in the province, the river Chiri overflows its banks, and spreads out into stagnant marshes at the three estates just mentioned; whilst lower down, the stream is almost altogether absorbed, and swallowed up in its sandy channel. From 6 a.m. to 6 p.m. you have all the region bathed in glowing sunshine; and, at sundown, an icy wind from the Cordillera blows over the heated plains underneath, so that no clothes can protect one from its chilling midnight effects. During the season of inundations near the foot of the Andes of Yupatera, the *malaria* on this property is so intensely active, that

it produces the most malignant remittent and intermittent fevers, which often prove fatal in the first, second, and third accession. In other instances, the endemic fever of this locality assumes the continued type, with a duration of two or three weeks; during much of which time the patient lies "atabardillado," or in an insensible state of fever with delirium. I have known an English overseer who was thus affected, and when he awoke into a state of consciousness, he found himself the only surviving inmate of his once happy home, his wife and children having been all suddenly cut off by the fever, whilst he lay insensible to the sorrows that surrounded him. The white inhabitants of the district who survive the endemic fevers, become subject to chronic disease and enlargement of the spleen and liver, while the negro race are the only human beings that thrive in it. The question of the special influences of race on the origin and spread of particular diseases, is a subject which deserves more attention than it has yet received. On the coast of Peru the negro resists the evil effects of malarial exposure far better than the Indian or European, and he also suffered comparatively little in the hæmagastric pestilence which recently committed great havoc among the other races. Cold notably modified the type of the recent epidemics, and though the temperature of Piura by day is dry and hot, yet it is a curious problem for the epidemiologist, what may be the effect of nocturnal cold on the type of malarial fevers in such a climate as that of Mono de los Padres, Moropon, and Yupatera? Has the gelid nocturnal breeze from the Cordillera the power of so modifying the miasm of the plains, as that it should in any instance be able to generate the continued, instead of the intermittent or remittent form of fever—and so run its entire course, as I have already observed, in the typhous shape? In the city of Lima, lat. 12 deg. 2 min. S., long. 76 deg. 58 min. W., six miles from the sea, and about 500 feet above the level, with a population of about 100,000 inhabitants, malarial and pythogenetic causes combined, are in co-operation, and therefore it is quite usual to see the intermittent fever pass into the typhoid; but the latter often occurs independently, and is one of the prevailing diseases of the capital, though pure typhus is rarely seen there. In the eminently dry province of Piura, the epidemic yellow fever of 1852 and 1853, did not reach the fatal climax of black vomit; and the deaths produced by it in certain Indian towns and villages, were chiefly owing to overcrowding in the ill-ventilated dwelling places of the poor. It was observed like-

wise, in southern Peru, that this epidemic did not, in 1855, diffuse itself within the dry and volcanic oasis of Arequipa: and though hundreds of cases were imported into the city of that name (lat. 16 deg. 20 min. S., long. 71 deg. 32 sec. W., at the foot of the snow-capped *Misti*), no new cases are known to have sprung from them. But beyond and above the richly cultivated and fertile oasis, and suburbs of Arequipa, as at Pocsi and Chihunta, for example, at the elevation of from nine to ten thousand feet above the sea level, and in a cool bracing climate, this pestilence established itself, and extended epidemically to the southern and inland departments of Puno and Cuzco; and proved most fatal in the city of Cuzco, which is situated at the elevation of about 11,300 English feet. At Cajamarca, lat. 70 deg. 7 sec. S., long. 78 deg. 31 sec. W., and at an elevation above the Pacific of 9,000 feet, the seasons were observed greatly to influence the force and spread of the epidemic yellow fever, in the years 1855 and 1857. Here the disease abated in the dry season, from May to October, and increased rapidly on the first appearance of gentle showers in the latter end of October. One morning, after some refreshing falls of rain on the preceding evening, almost all the cottages or native huts scattered over the extensive plain of Cajamarca, were revisited by the pestilence in the most sudden manner—and in front of every door there lay a corpse of livid or yellow hue, which but a few hours before, was the active living form of a human being: and in course of one month, we are told that Cajamarca lost 3,000 of its inhabitants from the ravages of this formidable disease. In other parts of inland Peru, however, as on the intercordillera table-land of Cerro Pasco, Janja, and Ayacucho, &c., the epidemic of 1855, much modified in its forms, had widely and rapidly spread abroad in the dry season of the Andes; and no success has, as yet, attended the attempts made to trace the source of this pestilence to any ascertained peculiarity of atmospheric condition. It is only certain that successive outbreaks of the same epidemic, on different sections of the Andes, were continued from 1853 to 1857-8, as a sequel to the grippe, which in 1851 commenced simultaneously all over the coast in the month of July, and thence traversed the whole Republic, highlands as well as lowlands, before the middle of September of the same year. This influenza, or grippe, was evidently dependent on some general atmospheric disorder, though too recondite to be made manifest to our ordinary senses; and I am not aware that any scientific meteorological observations were

made, in regard to the atmospheric or epidemic constitution of that year.*

It was from Lima and the seaports, that the various epidemics radiated to the interior in north south and central Peru. So that it must be always remembered that while the epidemic had retained sufficient vitality to climb the snowy Cordillera, strewing the mule road and footpaths, with the dead and the dying, it was not in the colder elevations it originated, but in the warm and humid climate of the coast, and in the heat of summer.

And it is certainly noteworthy, that wherever a new focus of infection originated (even in cases of ascertained contagion) the first symptoms of the malady were confounded with those of an ordinary catarrhal affection, so as to have very generally misled the doctors in the treatment, and especially as to the use of blood-letting, which was followed by rapid prostration of the vital forces. Independently, however, of all considerations respecting the origin of these and other Peruvian epidemics of older date, the diffusion and propagation of them, must have exhibited in their course, various climatic modifications, at the different well-marked zones of elevation above the sea, and particular variations of temperature connected with the epidemic region. As regards the eastern and western side of the same mountain chain, Humboldt remarked long ago, that people of a

* I may here allude to what I have fully related elsewhere (see Transactions of the Epidemiological Society of London, "On the Epidemics of 1719-20, and 1759, in Peru," part i, vol. 2) regarding the participation of animals, donkeys and dogs, etc., in some of the Peruvian epidemics. One of these fatal epidemics commenced in Buenos Ayres and extended inland to Peru, and was attended with strong hæmorrhagic symptoms; and the other overran the whole viceroyalty from Potosi to Quito, in an incredibly short period of time. The black cattle of the Sierra of Peru, if driven to the pastures on the coast in summer weather, are very generally attacked by fever, become stupified, and die with amazing rapidity of liver and intestinal disease, rendering the dead animal useless for the butcher. It is, therefore, a rule with cattle-dealers, to convey the Sierra cattle to the coast, during the colder months on the sea-board. Dr. Unanue of Lima, used in his Lectures, as I am informed, to give an interesting account of an epidemic among the huanacs, or huano birds at the Chincha Islands, where they were seen to rise into the air and drop suddenly dead in flocks at a time. To give an idea of the multitude of the birds and the quantity of ordure they are capable of yielding on a given surface in one year, I may just mention the following fact on the authority of my friend, Dr. Cuestas, who was living in Lima when I was last there in 1859 and 1860. He was a gentleman of great respectability, and owner of a small islet on the coast of South Peru, of about one hundred yards square. Before the foreign trade in huano, or guano, had commenced in Peru, this little property yielded annually 5000 fanegas, or bushels, of fresh huano manure, worth from four to five Spanish dollars per fanega. But during late years huanacs being much disturbed and diminished in numbers, the produce of this islet is reduced to less than 3000 yearly.

colder climate, when conveyed into the torrid zone, are readily affected by a hot and vitiated atmosphere, of which the natives scarcely experience the bad effects, and in illustration of this remark, he says that the great epidemics of Panama and Callao, used to be signalized by the arrival of ships from Chili, one of the healthiest countries in the world. (See *Essai Politique*, tome iv, ch. xii, p. 503.) To the same effect, a modern writer observes, that the climate of Lima, which has been always celebrated for its benignity, is not so in the present day. He thinks that the frequent and rapid communication with Panama by steamers, the immense ingress of Chinese and other foreigners, have impaired the local atmosphere of the capital; and consequently, since the year 1851, the population of the capital has been chastised by the frightful epidemic yellow fever, which as a rule, is a disease proper to hot and marshy climates, but which did not spare the sierra of Peru, but almost depopulated the province of Conchucos (of which the capital, Siguas, is in lat. 8 deg. 50 secs. S., long. 77 deg. 55 min. W.), and other provinces in the Department of Cuzco.* On the banks of the great inland rivers, Apurimac, and Pachachaca, in the south, and Maranon in the north, this epidemic assumed its gravest forms, and so tracing the march of yellow fever from the Port of Islay, on the Pacific, lat. 16 deg. 32 min. S., long. 72 deg. 43 min. W., across the pass of Santa Rosa to Sicuani, and the city of Cuzco, and thence to the sugar-cane growing valley of Abancay, lat. 13 deg. 37 min. S., long. 72 deg. 30 min. W., the Opuss over a track of ever-varying elevation and temperature of from four to five hundred miles in extent.

Since the preceding paragraphs were written yellow fever has again after an interval of twelve years, reappeared in a virulent epidemic form in the capital of Peru and its seaport Callao. In the Lima newspaper, *Comercio*, of 13th April, 1868, now before me, I find the following summary of deaths from the reigning yellow fever.

In the whole month of March 460 persons died in Callao of this pestilence, of whom 214 were natives, and 246 strangers, and among the latter, there were 110 Chilians, 38 Italians, 22 French, 17 English, 14 Germans, 8 Chinese, 7 Austrians, 5 Portuguese, 5 Mexicans, 5 Dutch, 5 North

* See book entitled "Geographia del Peru," by Dr. Don Mateo Paz Soldan, and edited by his brother, Don Mariano Felipe Paz Soldan. Vol. i, p. 281, and p. 437. Paris, 1862.

Americans, 4 Spaniards, 3 Ecuatorians, 3 Bolivians, 3 Greeks, 1 Swede, 1 Norwegian, and 1 Danish. In this enumeration Protestants, who were 50 in number, are not included. In all the month of March (1868) in the general cemetery of Lima (the capital) 161 who died of yellow fever were buried; and from the 1st to the 10th of April (when the list of deaths ends) 260 victims fell in Callao, and in Lima 150. The population of Callao is about a tenth of that of Lima, but in proportion the number of foreigners will be probably considerably more than in the capital, which may be estimated at about one hundred thousand, exclusive of transitory visitors; the epidemic broke out at Callao, and thence spread to Lima, where it is probable that the daily mortality would be for some time on the increase, until the winter set in about the end of May or middle of June. The epidemic has already spread some way to north and south of the capital; and the most striking feature connected with its diffusion is that while in the neighbourhood of Lima which is in 12 deg. 2 min. S. lat., the prevailing epidemic is yellow fever, in the province of Trujillo in 8 deg. 6 min. N., the epidemic is typhoid, as shown by the government despatches as follow:—

Lima, February 5th, 1868.—The government having knowledge that the Hospital of Trujillo is incapable for want of funds to render assistance to the poor, and considering that in this population an epidemic of typhoid fevers ("fiebres tifoideas") has broke out, which renders the immediate provision of the said establishment necessary, it is determined that the minister of state may give out the requisite instructions, to the effect that the Departamental treasury, remit immediately to the treasury of Trujillo 2,500 sols, with destination to the supply of the said hospital, &c., &c. Signed, MUNOS.

There is also another order of the same date and tenor from the same minister, Munos, addressed to the deacon of the medical faculty, on the appointment of a salaried medical gentleman to proceed forthwith to the bordering province of Chachapoyas beyond the Cordillera, "to combat the epidemic that has broken out there." And further, with date March 3rd, 1868, there is issued by the same authority a money order in favour of Dr. Adolpho Olachea, with a commission to combat the epidemic of yellow fever, "la epidemia de fiebre icteroides," which has developed itself, or broken out at the post of Tumbes, just on the frontier between Peru and Ecuador.

From these government commissions to various parts of the republic, we find that now, as in 1853, two modifications of fever—the typhoid and typhus icteroides—prevail in different sections of Peru; and in the present case we can hardly doubt that the same epidemic constitution reigns from Callao to Tumbes in 3 deg. 30 min. Between Lima and Trujillo there are over four degrees of latitude of coast, with a gradual diminution of atmospherical humidity as we proceed northward, and between the province of Trujillo and Piura there is the arid desert called Sechura of ever-shifting sand without water, and about one hundred miles in extent; and the banks of the river Tumbes, as we approach the bay of Guayaquil, is for ever verdant with the never-failing moisture of the Equator. In the dry central district of Trujillo, and in the remote transandine region of Chachapoyas, in a mild and equable climate, typhoid seems to reign; while on the Tumbes we have yellow fever, and the rainless and hot province of Piura to the south of the Tumbes, appears to be equally free of both forms of the epidemic, though the intercommunication must be constant. I wish I had the means of accurately collating the facts of these diverse climatic influences, with the precise modifications of fever prevailing in each, in order to be able to trace the phenomenal line of communication and natural connection between them. I hope to receive some further light on this subject yet from Peru. But in the mean time it may be sufficient to observe that on this as on former occasions, yellow fever came in the train of revolution and military commotion in different parts of the country, and ultimate massing of bodies of men in the capital, to which all converged from the outer provinces. The elements of infection probably abounded in the local atmosphere of Callao, the centre of shipping from all parts of the world, and met there suitable material of diffusion among non-acclimated, both natives and foreigners. The connexion between typhoid and yellow fever seems as prominent in the Peruvian epidemics of 1868, as in those of 1853-7; and in the epidemic aboard the *Jason*, on the West India and Halifax station, described in Dr. Slayter's valuable paper, read at a meeting of the Epidemiological Society of London, 2nd June, 1862, the connexion between yellow fever and various forms of typhus and typhoid fever seems indisputable.

ON CHOLERA IN PERSIA, 1866—68.

By E. D. DICKSON, M.D.,

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[*Read April 14th*, 1869.]

Constantinople, Feb 9th, 1869.

The brief sketch I have here drawn of the late outbreak of cholera in Persia, is derived almost entirely from the official records of the Constantinople Board of Health, to which I have occasionally added information obtained from other authentic sources, so as to render the text clear and connected throughout.

I believe the outbreak to have been a link of the widespread epidemic of 1865, which penetrated into that kingdom by way of Bagdad, on the return of the pilgrims from Mecca, as related in the report of the International Cholera Conference of 1866, which treats of the "March and mode of propagation of that Epidemic," and which shows the course followed by the malady as far as Suleïmanieh, and brings the date up to the 13th February, 1866. From thence cholera pursued its course into Persia, through the province of Azerbidjan, following a north-easterly direction, while at the same time it entered the Persian territory also by way of Khanikin to Kirmanshah, Hamadan, &c.

I have divided my subject into two parts. The first treats of cholera from its arrival in Persia as here stated, in the spring of 1866, until the cessation of the outbreak at Tehran, in the autumn of 1868, and forms the subject of the present memoir. The second part will contain a history of the recrudescence that followed it at Tehran in the month of November, and the march of the malady in a south-western direction towards the Ottoman territory, but which I am unable to complete at this moment, and must therefore reserve for a future occasion.

1866.—On the 21st June 1866, Dr. Rossi, Inspector of the Health Department at Erzeroom, informed the Constantinople Board of Health that cholera had broken out at

Maraga, a town situated on the Soujboulagh road, S.W. of Tabriz in Persia, distant about twelve hours from that city; and that ten days quarantine had in consequenee been applied along the Ottoman frontier, against arrivals from that place.

On the 2nd July, Mr. W. J. Dickson* (a brother of mine) wrote thus to me from Tehran: "Cholera is said to exist now at Soujboulagh near Oroomiah, whither it came from Kerkook or Suleïmanieh.

On the 7th July, the Governor General of Bagdad informed the Board of Health that cholera still existed at Bagdad, Mossul, Khanikin, and along the Turco-Persian frontier, and on the 24th of that month, His Excellency announced its presence also at Djeziré. On the 25th, the Board learned from Dr. Rossi that the malady was prevalent at Bitlis, a town near the south-western extremity of Lake Van, and on the 1st September Mr. Abbott, Her Majesty's Consul at Tabriz, announced to the British Embassy at Constantinople its appearance at Tabriz, and a recrudescence of the outbreak which had preceded it at Oroomiah. Further intelligence communicated by Dr. Rossi, dated Van, the 8th September, stated that cholera was raging at Eljivas, Altyntash, Kizil-diza and Soujboulagh, places in the neigbourhood of Lakes Van and Oroomiah.

On the 17th September, the French missionaries established at Kotur, a town situated between these two lakes, announced the appearance of cholera there, and added that it had been imported from Oroomiah, where the malady had shown itself in spring; that it had greatly diminished at Soujboulagh; was mild at Dilman, Djeziré and Kasrowa; but that it had passed from Selmas to Koï, where it was raging, as well as at Tabriz, and that it was moreover travelling towards the Russian territory.

On the 19th September Mr. Paduan, Inspector of the Health Department at Bagdad, wrote to the Board of Health that Cholera was disappearing from the Northern Provinces of Koordistan, but still existed at Bagdad, Suleïmanieh and Khanikin. Dr. Rossi notified on the 9th October, that it was approaching Erzeroom, having reached Boulak, a place only three days' distance from that city. On the 17th October, it had ceased at Van and Tabriz, but persisted at Moosh; and on the 31st, the malady had almost disappeared from Bagdad. On the 13th November, he announced its presence at Maku, and in the environs of Tabriz; and on the

* [Now Sir W. J. Dickson.]

2nd December he proclaimed its cessation at Bitlis, Akhlat and Boulanyk, but that it still dwelled at Maku, and in the plain of Moosh.

On the 5th December, the Ottoman Consul at Tabriz informed the Board that cholera had successively visited Nakhitchevan, Erivan and Tiflis, in Georgia, whence it had gone to Resht on the Caspian Sea, and to Kazvan, a town midway between Resht and Tehran; but that the outbreak at Tiflis, which at one time threatened to extend to Kars, had then happily subsided. On the 8th December, the Ottoman Minister at Tehran wrote to the Porte that the malady was advancing upon Tehran, and had nearly reached that capital; and on the 12th December, Mr. Paduan stated that it had visited Kirmanshah and Hamadan, cities in the west of Persia, lying on the road to Khanikin. On the 25th, Dr. Rossi announced its final disappearance from Maku; but that it persisted at Resht, Kazvin and Nakhitchevan. On the 28th December, the quarantine imposed on arrivals from Persia was taken off, cholera having ceased at Tabriz and its neighbourhood. While on the 4th January, 1867, Mr. Paduan announced four deaths from cholera at Ravendooz, on the Turco-Persian frontier; but which were not followed by any other cases.

1867.—A telegram from Nazim Efendy, Ottoman *chargé d' affaires* in Persia, dated Tehran, 28th August, 1867, announced to the Porte that cholera had declared itself at Tehran on the 23rd of that month, with a death-rate of sixty per diem. (The population of Tehran numbers about 150,000.) In consequence of this announcement, the Constantinople Board of Health sent Dr. Castaldi to Persia, as Ottoman Sanitary Delegate, in order to keep the Board informed of the progress of that outbreak, as well as of every circumstance connected with sanitary matters. A *cordon sanitaire* was moreover drawn between Bayazid and Van, and ten days quarantine applied to arrivals from Persia.

This outbreak is supposed to have come from Resht and Lahidjan on the Caspian, where cholera had been lurking for two months. Mr. W. J. Dickson wrote to me from Tehran on the 27th August, that cholera had spread itself in different parts of Mazenderan, and that he believed it reached Tehran from that quarter; and that it also existed in the sacred town of Koom, about eighty miles south of Tehran. It first broke out in the southern end of Tehran, which is the lowest and unhealthiest quarter of that city; and the

malady was at its height on the 25th August, on which day there were eighty deaths from it, and crowds of people were quitting the capital in great fright.

On the 17th September, it began to abate at Tehran; but broke out at Kazvin. Later news received at the Persian Legation, from Dr. Tholozan, the Shah's principal physician, dated Tehran 26th September, announced the cessation of cholera throughout the province of Azerbidjan; it stated moreover, that the malady had gone south to Kashan and Ispahan, and east to Meshed; while a subsequent telegram of the 13th October informed the said Legation that cholera had quite disappeared from Tehran, as well as from Ispahan; and that no traces of it were left at Hamadan, Kirmanshah or Tabriz. "In fact," added Dr. Tholozan, "I believe the malady has entirely gone from Persia, except at Astrabad."

This announcement caused the *cordon sanitaire* to be suppressed on the 26th October following.

"Thus (reports Dr. C. Millingen, quarantine physician at Bagdad, to the Constantinople Board of Health) cholera quitted Tehran in September, 1867; spread from place to place, along the principal lines of communication towards Kashan; and in December it reached Bourndjird, a place through which pass the caravans on their way from Tehran and Ispahan to Dizfool and Shooster, on the banks of the river Karoon. In January it was propagated to Dizfool, where it carried off in the space of a month nearly three hundred persons, out of a population of twenty thousand souls. Then it attacked the Arab tribe called 'Beni-Lam' on the S.W. of Dizfool, while at the same time it advanced from village to village in an easterly direction, until it reached Shooster, where it destroyed about six hundred persons out of a population of eight thousand. There appeared at the same time (adds Dr. C. Millingen) at Howiza, on the river Kerkhah, three days' journey from Dizfool, a formidable outbreak of malaria fever, which lasted from September until the end of January, and carried off nearly six hundred persons out of a population of only seven thousand."

1868.—On the 29th January, 1868, Dr. Castaldi informed the Constantinople Board of Health of a severe outbreak of cholera at Balfroosh, a town near Astrabad in the province of Mazanderan, and in a subsequent report dated the 11th of March, he remarked that the persistence of cholera in Mazanderan, and at Resht, during the whole of that

winter, could not but awaken serious apprehensions with regard to the public health of the ensuing season; the more so as that province was in a most deplorable state, and presented according to Dr. Tholozan, who had frequently visited it, climatic and local conditions very similar to those of the choleraic districts in India.

"Cholera (added Dr. Castaldi) has never left the province of Mazanderan since its appearance there last year. Repeated outbreaks at Balfroosh, at Amol and at Sari, show the tenacity with which it clung to a locality that favoured its existence; and this circumstance was not limited to Mazanderan, for a similar course was observed in the town of Meshed, where repeated outbreaks of the malady had also manifested themselves."

Meshed is the resort of a pilgrimage; and devotees from every part of Persia yearly throng to its shrine. The multitude which gathers there, and the filth engendered by the crowd are sources capable of evoking epidemics and especially outbreaks of cholera; while the return of the pilgrims to their homes, from this infected locality, tends to disseminate the malady in every direction where they go. "Such in fact (says Dr. Castaldi) has proved to be the case this year; for when cholera had spread from Kooshan to Meshed, and the mortality had risen there to one hundred per diem, on a population of about seventy thousand souls—the Shah was, for the first time, apprised of its existence; and it being then the commencement of August—that is, the moment when the pilgrims begin to disperse from Meshed, His Majesty, in order to preserve his capital from contamination, issued directions that those pilgrims who intended to return to Tehran, would have to undergo quarantine at a spot distant twelve days' journey from the capital; but this order was disregarded, either (as often happens in Persia) through the neglect of those charged with carrying it out; or because the pilgrims, to avoid the measure, had altered their course, and thus evaded altogether the quarantine station."

It was on the 6th August, 1868, therefore, that a party of these pilgrims entered Tehran without performing any quarantine. At the same time a regiment of soldiers coming from Astrabad, met on the road another caravan of pilgrims returning from Meshed, amongst whom cholera had been raging. They mingled together, and the soldiers soon caught the disease. On reaching Tehran, their entry was forbidden, and the soldiers disbanded; but they found means of

penetrating into the town, and on the 7th August the first fatal case of cholera occurred at Tehran, upon the sister of a pilgrim who had arrived from Meshed, named Ali-khan, who having lost his wife on the road, quitted the caravan, and was one of the first persons who entered the town.

During the following five or six days, vague and contradictory reports circulated with regard to the disease; and it was not until the 14th that the existence of cholera was ascertained and officially announced. A registry of the deaths was then kept by Mirza-Riza, governor of the town, from which it results that during the first four days, the malady had only carried off eleven pilgrims; but on the 11th August it began to spread amongst the population, and from this date the mortality increased until the 20th, on which day there occurred sixty deaths from it. On the 21st, twenty deaths; and on the 22nd, fifty deaths. Total, from the commencement of the epidemic, up to this period (22nd), three hundred and forty-three deaths. From this time to the end of August, the death-rate from cholera oscillated between nine and fifteen a day. During the first week in September, it varied between eight and eighteen deaths per diem; from this to the 19th, eight to twenty-seven deaths per diem; from the 20th to the 30th, two to eleven per diem; and from the 1st to the 11th September, one to three deaths per diem.

No further deaths from cholera were noticed at Tehran, after the 11th; but, on the other hand, the epidemic had spread during the latter part of August to the neighbouring villages of Djaferabad, Zerghendek, Goulahek, and Simeran; towards the end of September it reappeared at Sari, a town in the province of Mazanderan; and in the beginning of October it broke out at Yezd, where it was carried by the pilgrims returning from Meshed; and where it is said to have committed great havoc.

Until the 20th August, the malady was restricted to the filthiest quarters of Tehran, called Dervesenan and Dervesedolab, where it had in the first instance broken out; and where the caravanseraï resorted to by the pilgrims were situated. But after that date, it spread into other parts of the town, invariably committing its utmost ravages in those places where poverty and crowding were greatest. (Castaldi's *Report to the Board of Health*, 21st October, 1868).

"The same panic (says Dr. Castaldi) which seized the population of Tehran last year, showed itself also upon this occasion; and entire families fled from the capital, convey-

ing the infection to the neighbouring villages, as proved by the manner in which cholera was introduced into Larghendek and Djaferabad."

Nothing could surpass the indifference of the Persian Government, on a momentous event like that of the present outbreak; and no efforts of any sort were made, either with a view to arrest the progress, or mitigate the evils of the malady.

Whether the epidemic was propagated to Meshed from Mazanderan, where it had been lurking since last year, and would thus be considered a continuation of the same morbific influence;—or whether (as some persons imagine) it was a new importation from India, by way of Afghanistan where it is said to have been raging; it certainly arrived at Tehran just at the season most favourable for its development,—when bowel-complaints prevailed; when unripe and rotten fruit were plentiful; when the inhabitants slept upon their terraces, in the open air, exposed to a high temperature during the early part of the night, which afterwards became chilly towards morning; and when the water-supply was scanty, and stagnated in the open gutters of the streets. "The Persians (observes Mr. W. J. Dickson, who has lived many years in their country) have only to blame themselves, for the frequent recurrence of cholera. Were their habits more cleanly, this scourge would surely not show itself so often upon these elevated and naturally healthy regions!"

On the 5th September, Dr. Castaldi wrote to the Board of Health that up to that date no manifestation of cholera had shown itself at Kazvin, Zendjan, Tabriz, Hamadan, or Kirmanshah; and that hence the Turco-Persian frontier was not menaced with an invasion of the disease. In fact, no restrictive measures were, on this occasion, adopted by Turkey against arrivals from Persia, and the result has shown the wisdom of abstaining from them, when the intervening distance is so great, and the chances of success from a *land-cordon* so small!

Dr. Tholozan declared, on the 28th September, to the Constantinople Board of Health, his conviction of the utter uselessness of maintaining a sanitary cordon from Ararat to the Persian Gulf, to arrest the progress of cholera; and he founded his opinion on a careful study of the history of past epidemics in Persia. He argued that in consequence of the nomadic character of the inhabitants, who, half Persian and half Ottoman, are only nominally under the control of either power, it would be extremely difficult to organize an efficient sanitary cordon to protect this frontier.

On the other hand, Dr. Bartoletti (one of the Ottoman members of the Constantinople Board of Health, who has visited that line of frontier) maintains that this control is feasible, provided the two nations carry it out conjointly. He asserts that there are but five roads or defiles through which the pilgrims (who in fact are the carriers of cholera) pass in their journeys from one territory to the other, viz., Eneshka, Betwin, Nalparez, Khanikin, and Mendeli; that cholera travels in Persia by means of the perpetual to and fro movements of pilgrims; and that the cordon had hitherto failed to arrest its progress into Turkey, owing to the opposition met from these very pilgrims, who in every instance had forced their way through the cordon; and to the indifference shown in such cases by the Persian Government, who alone had the power to coerce them into an observance of the restriction. Dr. Bartoletti added, that the multitude of pilgrims who journey from Persia to Bagdad in the month of Moharrem, has sometimes constituted a mass of not less than sixty thousand persons; and that during the twelve years he had watched these events, cholera had crossed the Turco-Persian frontier eight times, and this always at Khanikin.

Before concluding, I beg to state that the Persian Government, awakened to a sense of the necessity of giving attention to sanitary matters, has created a Board of Health at Tehran, under the superintendence of the Minister for Foreign Affairs.

The object of this institution (I am told) is to consider the sanitary questions relating to Persia, which have been discussed by the Cholera Conference of 1866; and to frame rules for the preservation of that kingdom from future invasions of cholera.

NOTES ON THE CHOLERA EPIDEMICS IN EAST AFRICA.

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[*First Paper. Read January 11th,* 1871.]

1835-36. *The First Epidemic of Cholera* in East Africa to which any definite date can be fixed occurred in Dec. 1835 [1836*] and Jan. 1836 [1837*], during the prevalence of the north-east monsoon, and its line of progress was in the direction of the monsoon from north to south. It was first heard of in the Somali ports and gradually extended southwards in the trade line till it reached the Island of Zanzibar, and from thence it passed further south; but its limits in that direction are unknown. Arabs, who have a distinct recollection of this epidemic, all agree regarding its line of progress, and although the mortality was trifling as compared with succeeding epidemics, it was sufficient to impress the memory and fix the date. In speaking of this epidemic they generally say that it only lasted a few weeks; that the deaths were among the slaves only, and that but few Arabs died, the epidemic being regarded as mild, only in so far as it affected themselves. In Zanzibar there are many natives of India who recollect this epidemic, and they speak of it not only as coming from the north but as being of African origin, and make a distinction between African and Indian cholera; but this distinction arose probably from the fact that while in India their race suffered severely from cholera epidemics, they, in Zanzibar, were almost entirely unaffected by this and succeeding epidemics; so that while warning them regarding the danger to which they were exposed, while the present epidemic was progressing towards the island they spoke lightly of it, and said that it was only African cholera.

Between the years 1835-36 and 1859 there was no epidemic of cholera in the Zanzibar territories. In 1856 small-

* [See Second Paper.]

pox swept off vast numbers of the population, but with this exception there was nothing unusual in the health of the population.

1859-60 [1858-59*]. *Second Epidemic.*—This epidemic, like the first, appeared in December and January during the strength of the north-east monsoon, and it was also first heard of in the Somali ports of Mukdesha and Brava. It may be necessary to mention that from December to March the wind blows steadily from north to south, and from June to October from south to north, the former period being called the north-east monsoon and the latter the south-west monsoon. During the strength of the north-east monsoon there is rapid communication between north and south, but none from south to north, and *vice versa.* The epidemic, therefore, being present at the northern part of the Zanzibar dominions at the setting in of the northerly monsoon of 1859-60, at the very time that communication towards the south was opened, passed rapidly in that direction, desolated the coast towns and the populous islands that fringe the coast, extending as far as the Portuguese settlements at Mozambique. Dr. Kirk, who was at that time stationed at the Zambezi in connection with Dr. Livingstone's expedition, states that it stopped short of that district. This was probably owing to the change of the monsoon arresting communication further south than Mozambique. General Rigby, who was then Her Majesty's consul and political agent at Zanzibar, states that many of the coast towns were almost decimated, and that in the town and suburbs of Zanzibar the mortality was from 7,000 to 8,000, and in the whole island about 20,000. During this epidemic very few natives of India died, only three or four; although at that time they were more closely mixed up with the slave population than now. The mortality amongst the Arabs was much greater than during that of 1835-36, but the native population, especially the slave, suffered most. The epidemic spread inland from several of the coast towns, but its range was limited, and no great mortality was heard of in the interior. It did not remain long in the island or on the coast.

1865. *Third Epidemic.*—The third epidemic of cholera, on the east coast of Africa, was first heard of in April 1865, in the Somali Ports. This being towards the close of the north-east monsoon, its progress towards the south was limited as the south-west monsoon set in early and strong, and shut off all traffic to the southward. Lamoo suffered very

* See Second Paper.]

severely and also Melinda. A few cases occurred at Mombassa, but the disease did not advance further south. At Melinda it struck off to the interior and prevailed to a considerable extent in Ukambani and further inland. At Patti and Lamoo, where the greatest mortality took place, the epidemic lingered till June. Baron Von der Decken, who was then engaged in the exploration of the coast contiguous to the river Juba, lost several of his men from the disease.

The honorary secretary of the Epidemiological Society asks specially "whether cholera prevailed in the island or anywhere in its vicinity in 1864 or 1865." In 1864 there was no cholera on the east African coast to the southward of Mukdesha, but that there was in 1866 is rendered certain by the letters of Baron Von der Decken, who was in the locality at the time, as well as from the accounts that reached Zanzibar regarding the great mortality at Lamoo.

It may be necessary here to explain that Mukdesha is the farthest northern port on the east African coast which keeps up, every season, direct communication with the Island of Zanzibar, and that it is extremely difficult to get any information regarding anything pertaining to the state of the population further north than this. We know nothing of the three epidemics beyond this point. It is extremely improbable that they originated at the Somali ports or in Somali land, but whether they proceeded from port to port along the coast or from the interior we know not. It is to be hoped that by supplementary information from other quarters the epidemic of 1865 may be traced further north to the Persian Gulf or India, and this may indicate the origin of the others.

1869-70. *Fourth Epidemic.*—The fourth epidemic of cholera on the east coast of Africa and the third in the Island of Zanzibar occurred in 1869-70. As more is known regarding this than any of the three preceding epidemics, and as it is somewhat unique in regard to its origin, course and symptoms, we will consider it more in detail, and sketch first its progress in the territory generally, and then in the town and island of Zanzibar.

Towards the end of October 1869, news reached Zanzibar that caravans in passing through the country of the Wamasai were attacked by a deadly plague; that some of them were disabled to such an extent, that the merchandise was abandoned and the survivors fled in terror towards the coast, and that the disease was raging in the Usambara country through which they passed, and also at the coast towns of Tanga and Pangani.

The country occupied by the Wamasai lies to the west of Usambara, and is represented in the maps of Africa by a blank. Little is known of the people themselves and still less of their country, as it has never been visited by Europeans, and we depend for our information regarding them on passers through the country, the boundaries to the north, south and west being entirely unknown. The Wamasai are a pastoral people; they have no villages and move about from one district to another according to the season to find pasturage for their flocks. In manners and customs and probably in origin also, they are closely allied to the Mavite who inhabit the country to the north-west of Lake Nyassa. They are represented as being an exceedingly fierce and warlike tribe, the terror of all their neighbours, and during times of scarcity they make raids on the coast towns, devastating the country in their line of march, and carrying with them all the cattle. A few years ago they harried the country between their own territory and Mombassa, and even threatened the town. To say that the Wamasai are out is sufficient to strike terror to the hearts of all their neighbours and make them fly from their line of march.

Pangani and Tanga are situated a little to the north of the Island of Zanzibar, and are the starting points for caravans proceeding to the interior towards the Victoria N'Yanza and further to the north. These caravans skirt the Usambara country in their course and pass through the territory of the Wamasai, the larger caravans paying the usual blackmail levied, and the smaller exchanging commodities and then returning to the coast towns. Beyond the Wamasai region there is as yet no trace of the epidemic, and unless it originated amongst them it must have proceeded along Central Africa from the north or across the country from the west, as it would certainly have been heard of had it crossed the Nyassa or Uniamuya caravan route.

Communication by dhows between Tanga, Pangani and Zanzibar, is of almost daily occurrence, so that the epidemic speedily reached the island. And it certainly did appear on the 27th of October, if not a few days sooner.

Zanzibar may be described as the centre of all the commerce of east Africa, from Cape Delgado to Gardefui. All the merchandise sent to the coast towns and from thence by caravans to the far interior proceeds from its harbour, and all the return products pass through its custom house, and amongst other products from 25,000 to 30,000 slaves annually. No precautions whatever were taken to prevent the epide-

mic from gaining a footing in such an important centre; none to prevent its spread in the vast territories with which it is so closely connected: and nothing was done to arrest the progress of the disease in the town and country or to alleviate the sufferings of the wretched slave population who died in thousands during the first six weeks. Towards the end of November and beginning of December, the town and harbour of Zanzibar were literally saturated with the cholera poison. All business was suspended; deaths were taking place in unknown numbers; the European merchants had difficulty in carrying on their business for want of labour, as the living were engaged in burying the dead; the entire population was panic-stricken, and towards the middle of December, when the mortality reached its highest rate, the people lapsed into a state of absolute despair. Tired of burying their dead, the negroes exposed them in the bush or threw them over the bridge to be carried away by the tide, as I have myself frequently seen while passing during the night; and those who died in dhows were thrown over-board in the harbour. The slaves usually bury their dead in the sand close to the beach and in the vicinity of the town. Even in ordinary times they merely scratch up a little loose sand to the depth of a foot or so, introduce the corpse and sprinkle it over with a little sand, which is washed away by the tide or removed by the wild dogs at night when they come for their evening meal. At this place it is quite common at all times to see half devoured dead bodies lying exposed. During the month of December the stench from bodies in all stages of decomposition was so overpowering as to render the place unapproachable, and thus Zanzibar disseminated the very essence of the cholera poison to all the coast towns and adjacent islands both towards the north and south. In all the Sultan's dominions where there was open intercourse with Zanzibar, the disease appeared in its worst form. It passed slowly towards the north against the monsoon, and was conveyed to Mombassa, Melinda and Lamoo in slave dhows. From Mombassa it passed into the interior among the Wanika, but the mortality was not great. The arrest of its progress further north than Lamoo seems simply to have depended upon the circumstance that the strong north-east monsoon effectually blockaded the Somali ports by shutting off all intercourse for the time; for towards the end of April, when the monsoon changed and trade was opened between Zanzibar and these ports, the epidemic appeard at Brava with great virulence, and reports have recently reached us

that half the population has perished. Towards the end of December the deaths at Melinda were estimated at 2,000, but at Lamoo the mortality was said to be small compared with that during the epidemic of 1865.

The progress of the epidemic from Zanzibar towards the south was much more rapid, and in the course of a short time it raged in all the coast towns as far as Cape Delgado, the southern boundary of the Zanzibar dominions, beyond which it did not pass at that time. Zanzibar dhows, however, conveyed it to the Comoro and Johanna Islands, but Ibo, Mayotta, Nossi Be and Mozambique escaped, although they usually carry on at this season a large trade with Zanzibar. This may be accounted for by the fact that there was open communication with Comoro and Johanna, but that at the other ports there was strict quarantine. The epidemic did not appear at Mozambique till so late as the 6th of May, and in all probability while the sea-board was guarded by strict quarantine the disease may have spread along the coast from the neighbouring Zanzibar territory. That the rapid spread of the epidemic towards the south did not depend so much upon the direction of the monsoon, in itself considered, as in the fact that the Zanzibar dhows taking advantage of the favourable winds made rapid runs to their several ports of destination, appears probable from the following case. On the 20th of January the crew of a Portuguese brig which was to sail on the following day for Mozambique were attacked with cholera. In the course of a few days seventeen were seized. They were all removed on shore and the vessel was cleansed and fumigated. Four deaths occurred, and those who recovered went on board the day before sailing, the 6th February. The vessel reached Mozambique in about eight days, and having a foul bill of health was put into quarantine for a month. No fresh case occurred on board ship and she was allowed to come into the harbour. Had this vessel put out to sea as the native craft invariably did, and been allowed to communicate with the shore, the epidemic would certainly have appeared in Mozambique in the month of February, as it did then or previously in all the ports at which Zanzibar dhows arrived and in which no quarantine regulations existed.

In Quiloa, a small island close to the coast, and to the south of Zanzibar, the mortality was excessive, averaging in January as many as two hundred deaths daily. This island is a centre of commerce to a large district, and is the place to which nearly all the slaves from the interior are

brought, and from which they are shipped in dhows for Zanzibar; and when we consider that from 25,000 to 30,000 are driven annually through this narrow gateway, and that when they arrive at Quiloa their physical condition after many months of toilsome march and starvation baffles description, we may imagine the fearful mortality that must have taken place amongst these living skeletons. The prices were so low owing to the great mortality, that purchasers could not be found willing to pay a dollar for a slave, and hence they were allowed to accumulate and die of starvation and cholera. The mortality amongst these miserable beings at Quiloa will never be known, but it must have been unexampled in the annals of all epidemics.

We have but little information regarding the progress of the disease towards the interior of Africa, but from what we do know there is reason to believe that every town on the mainland became to a greater or less extent a new centre of dissemination. From Bagamoyo, nearly opposite Zanzibar, it followed the great ivory caravan route towards Uniamezi, and raged in Usegura, Usagari, Ugogo and Mgunda Mkhali at an early period of the epidemic, and letters recently received from Uniamezi report that it is now prevalent in that district. It appears that the regions mentioned are not yet free from the epidemic, for the porters of a caravan from Uniamezi which arrived at Bagamoyo about a week ago (16th August 1870), were attacked by cholera in Usagari and six were said to have died at the coast, thus bringing the disease back to within twenty-five miles of Zanzibar. During the height of the epidemic, when caravans from the interior reached the infected districts and were attacked by the pestilence the survivors refused to proceed, threw down their loads of ivory and fled to their homes, while the leaders of the caravans had no choice but to conceal their valuable cargo in the bush, hurry on to the coast and await the result. For the same reason porters could only be collected with great difficulty, and sometimes they could not be got, to carry merchandise to the interior; and when cholera appeared amongst them in the infected districts the loads were thrown down and the caravan completely disorganised. On two occasions supplies forwarded to the interior for the relief of Dr. Livingstone could not be passed through the infected districts, as the bearers were attacked with cholera.

The Rev. L. Fraser of the "Universities Mission," was in Usambara throughout the epidemic, and he reported the mortality as having been very great among the people and

the symptoms as being very severe. At first he could not understand the nature of the epidemic, as the people seemed almost to drop down dead. At the solicitation of his friends he returned to Zanzibar, and although he had been well while in Usambara, he was seized with cholera on the very day of his arrival and died within a week. The Rev. C. New of the Free Methodist Mission, whose station is amongst the Wanika, about fifteen miles to the west of Mombassa, had not heard in May of the advance of cholera to the west of the people amongst whom he resides.

In all the turnings and windings of the cholera epidemic there seems to have been one unvarying principle directing its course. It has invariably accompanied trade wherever its direction may have been. It came from the interior and returned to it in a parallel line; it advanced to the north and was intercepted by the monsoon blockading the harbours further north, and its progress to the south was intercepted, at least temporarily, by the quarantine at Mayotta, Mozambique and Nossi Be.

Progress of the epidemic in the town and island of Zanzibar.—The progress of the epidemic in the town and island was very marked, and it may be stated, generally, to have spread from the lowest to the highest classes of the population, dividing it into distinctly marked periods. The population of Zanzibar is comprised of Negroes from every tribe and country, Arabs from Sheher and Muscat, settlers from the Comoro Islands, natives of India, Turks, Persians, Beloochees, Abyssinians, Goanese, Europeans and Americans.

All these different classes stand out distinct from each other with a greater or less degree of prominence, and although all are mixed up in matters pertaining to business, they are entirely distinct in all their social relations, and they are congregated in different quarters of the town.

The first period of the epidemic was amongst the Sheheri Arabs and their slaves, the slaves being attacked first. It appeared first in the house of a Sheheri Arab who had some connection with the dhow which came from Pangani and anchored in the harbour of Zanzibar on the 27th of October. He is the principal owner of the hamalis or porters, a large number of whom are constantly employed in loading and discharging vessels, and in carrying goods from one part of the town to another, there being no other means of conveyance.

The Sheheris reside in one district at Melinda, and,

forming the poorer section of the Arab community, are employed as mat makers, butchers, soldiers, and many of them, when not otherwise employed, work along with the hamalis as porters. They suffered first and very severely; indeed to such an extent was their number cut down, that their work for a time came to a standstill.

During the second period of the epidemic, from the middle to the end of November, the entire slave population throughout the town, the Comoro and Johanna people, and towards the end of the month the Muscat Arabs, were attacked by the disease. The people from Comoro and Johanna, a mixed race of Arabs and Negroes, have settled in considerable numbers in Zanzibar, and with their slaves inhabit their own quarter. Being more intelligent than the negroes, and very poor when they emigrate to Zanzibar, they are employed as head-servants and overseers, and being keen slave-dealers, they invariably invest their savings in slave property, generally boys and girls, who are employed in large numbers in picking orchella weed, copal washing, and such light occupations.

Every one owns a few slaves and many of them considerable numbers. The quarter which they and their slaves occupy is strictly native. They live in huts thatched with cocoa-nut leaf, and divided by internal partitions into small rooms, and the utmost care seems to be taken that neither light nor air should enter the room. Even during the midday glare of a tropical sun, the house is as dark and stifling as a dungeon. The houses are set down without any regard to order, or as if the arrangement had been intended to prevent the possibility of a free current of air from reaching any part of them. The slaves of these people are poorly fed, the masters only allowing them about three half-pence a day for their food, and their condition is much below that of the slaves of the Sheheri Arabs, who are well fed, strong muscular men. The mortality amongst this class was very high, and at least 50 per cent. of the slaves died during the epidemic, and of the masters and their families not less than 15 per cent.

During the month of November (except in three cases) the ravages of the epidemic did not extend beyond the Arab populations, beginning first with the slave population; and the deaths occurring in districts apart from that of the Europeans, little was known of it except by reports, and these were considered to be exaggerated, as few of the symptoms usually attending cases of cholera were described.

During the month of November, the temperature was very high and the winds were light, so that the heat was excessive for Zanzibar, and it was at first supposed that the cases of sudden death had taken place from sunstroke or heat apoplexy amongst those whose occupation was in the open air and hot ill-ventilated sheds. The hamalis and coolies at work in the open air, and the slaves engaged in the picking of orchella weed, were suddenly seized with vertigo; fell down in a state of stupor, and were carried to their huts, death ensuing in a few hours. Every one seized seemed to know instinctively the deadly nature of the seizure, and they staggered to their homes to die. The lightning-like rapidity of the seizures, and the rapidity with which death followed, filled the negroes with consternation, and observing that none of the Europeans had been attacked, from some superstitious reason or other they commenced to white-wash the bodies of the dying, to make the evil Spirit, who was supposed to be the author of their calamities, believe that they were white men; but the epidemic continued its ravages among the coloured population, the Sheheri and the Muscat Arabs, and the people of Johanna and Comoro till the end of November.

The third period of the progress of the epidemic occurred at the end of November and beginning of December, when the natives of India were attacked. These people are more distinctly separated by their usages from the general population, than any other section of the community. They themselves are separated into three classes, the Khojahs, the Bahoras and the Banyans, the two former being Mahomedans of different sects, and the latter Hindoos. Numerically the Khojahs are the most important class, there being about two thousand resident in the town; the Banyans rank next, while the Bahoras form a very small community. These three classes are entirely distinct in all their social relations, the Banyans being more isolated from their fellow countrymen and the general community than any other class. They are vegetarians and only certain edible vegetables are eaten. They are exceedingly particular regarding the water they use, but not in regard to its quality or purity. In Zanzibar they draw it from a well of their own in town, and nothing would induce them to drink water, or even touch it, if not drawn by themselves. In regard to diet they are also very fastidious. They eat by themselves and in secret, and should a stranger enter while at a meal, it is considered to be contaminated by his presence. Nothing left over from

a meal is kept, and the leaf platters which they use are also thrown away. They burn their dead, and also the clothes of the deceased person, and although their houses are far from being clean in the European sense of the term, yet they are generally free from putrid decaying matter. In none of the former epidemics of cholera was the Banyan population afflicted, and in that under consideration although there were a few deaths, it never spread amongst them as an epidemic. The same may be said of the Bahoras, of whom several died, but only two were resident in the place, the others being strangers from Pangani, Pemba and India. We shall refer hereafter to the great mortality among strangers.

It was the Khojah class then that suffered most severely or most prominently during the month of December, the disease still continuing its ravages among the other classes of the population, and reaching its greatest intensity about the middle of the month.

The Khojahs in Zanzibar are nearly all from one district in India, and they are intimately mixed up by marriage relations, even cousins and second cousins being regarded as near relatives, and forming family ties.

As a class they are exceedingly kind and affectionate towards each other, more so than I ever imagined any people could be, and in a case of serious illness, all the friends hurry to the house, sometimes crowding it to suffocation, and there is a constant stream of comers and goers. No one could fail to admire the devoted kindness of these people towards each other, but, although warned against the danger from contagion, custom prevailed, and panic-stricken they hurried by day and by night to the houses of their suffering relatives, and too often carried to their own homes the fatal disease under which they themselves fell victims. Knowing the Khojah population intimately, it appeared evident to me at the time, that the great violence of the epidemic amongst them resulted from such intercourse. This however was not always the case, at least not evidently so, as the epidemic, even amongst them, moved about from place to place in the most mysterious manner. At one time all the new cases would occur within a very limited range, in one district of the town, and again at some other, and on some occasions it would appear in three contiguous houses on one side of a street; on others in two on one side and in one on the opposite.

Towards the close of December the disease gradually

abated, and hopes were entertained that it had ceased as an epidemic, only a few isolated cases occurring here and there thoughout the town; but during these terrible two months, there fell in the town and suburbs from 10,000 to 15,000 people, at least 12 per cent. of the whole population having perished, and amongst these, out of a population of 2000, 100 Khojahs.

During the fourth period of the epidemic, the disease spread throughout the island generally in all directions, and appeared in its greatest intensity during the month of January. The rate of mortality was not nearly so high in the country as it was in town, the people being as a matter of course more scattered. At the beginning of January, cholera appeared on the estate of Messrs. H. A. Fraser and Co., at Kokotoni, situated at the northern part of the island, and at the close of the epidemic the mortality was estimated at 5½ per cent. Great care had been taken to isolate all the cases, and every means used to prevent the spread of the disease, and all those attacked were under medical treatment. As no such precautions were used on the estates owned by Arabs, the deaths among their slaves must be estimated at a much higher rate.

From the daily intercourse between town and country, in all directions, the spread of the epidemic to every corner of the island can easily be accounted for. Starting from the shambas early in the morning, and from distant estates at midnight, numbers of slaves make their way towards the town, loaded with the produce of the country, and converge at the market places, which from seven till nine o'clock are almost impassible. Threading their way through the dense mass, the ditals or auctioneers dispose of their wares to the highest bidder, the slaves who sold a few vegetables on their own account being the buyers. Slaves who have any article of clothing to dispose of, such as a cap or loin cloth, dispose of it in this manner. During the month of December, when the mortality was at its highest, the clothing of the dead thousands was thus sold in the markets, and I have no doubt that by this means the disease was sown broadcast over the island. If the filthy rags could be bought for a few pice they were speedily purchased, and were put on at once as articles of clothing.

In so far as the natives of Zanzibar were concerned, or more strictly speaking, those permanently resident on the island, the disease ceased towards the end of January as an epidemic, isolated cases occurring here and there through-

out the town and country. The actual mortality, however, was sometimes very great, leading one to suppose that the epidemic had broken out afresh; but on investigating the cases, it was found that it was strangers who had recently come to the island who suffered. This leads us to consider the progress of the epidemic among strangers, viz., the crews of vessels in the harbour and of native craft passing to and fro.

The Cutch and Bombay dhows, and various native craft from the Arabian shore of the Persian Gulf, come to Zanzibar during the north-east monsoon, and towards the close thereof the Somalis make their appearance, increasing the population very largely. Dhows also are constantly running to and from the mainland, so that the passing population during the months of January, February, March and April is very large. From the commencement of the epidemic the mortality was always high in the harbour, especially among the native craft, which are unsurpassed in regard to filth and every abomination, and they appear to have communicated the disease to European vessels even in passing. Early in December, and a day or two after sailing, cholera appeared in H.M.S. *Dryad* but only one man died, a native boy having been attacked first. In the middle of December, a merchant vessel was attacked, and in a few days six out of nine of a crew died, while many native craft had to be abandoned. Starting with cholera actually among the crew, or appearing soon after sailing and spreading rapidly, they ran for the nearest port or put back disabled. Towards the end of January, the crews of three European vessels in the harbour were attacked at nearly the same time, and in the case of two at nearly the same hour. A building, the property of the Government, was kindly given to be used as an hospital for the use of the shipping by Dr. Kirk, H.M. acting Consul, and from that time all the cases which occurred were promptly removed on shore, and treated to a termination. During the months of February and March, the epidemic prevailed in the harbour, and several cases occurred on shore among the wealthier Arabs. Several dhows running to Madagascar had to put back disabled, and in March the loss on native vessels reached its maximum. In April the epidemic broke out at Bagamoyo amongst the natives in connection with the Roman Catholic Mission, and in Zanzibar amongst the Bahoras; but those who were attacked were all strangers from Pangani and India. The mortality was also very great among the Somali strangers,

and was principally confined to them, the deaths from cholera being about thirty daily.

During the month of May very few cases appeared in town, but the mortality in the harbour was still great, owing to the disease having been brought back from Quiloa in slave dhows, and this state of affairs continued till the end of June. In the middle of July the last case that I heard of occurred.

In connection with the epidemic in May and June, when the disease was continued in the harbour by the cholera being returned from the slave mart at Quiloa, I cannot but mention the shocking condition of the slaves imported to Zanzibar at that time, although this is somewhat apart from the subject of consideration. I speak of nothing in connection with the subject of which I was not an eye-witness and which I did not note at the moment. During the first two weeks in May, several cargoes of slaves were landed at the custom house in the most horrible condition imaginable, and I believe that it is only at the harbour of Zanzibar that a living skeleton, reduced to that state by compulsory starvation, can be seen. On the 17th of May a cargo of slaves in this condition was landed at the custom house. I was informed and I have every reason to believe correctly, that about 250 had been put on board at Quiloa with only a little food and no water, in a very small dhow. I saw the dhow coming into the harbour with its living cargo crowded together as close as people could stand or could be packed. The dhow had been three days on the passage and cholera was said to have broken out on board, seventy having died. The duty on slaves is paid on their passing through the custom house at Zanzibar, so that to save that expense the dying wretches were left to expire on the sands, and the living skeletons were driven and dragged and kicked along past my door in open light during the forenoon, and a scene such as this excited no surprise. In the evening of the same day one of the cargo, a female, was picked up by two other gentlemen and myself at the outskirts of the town while staggering towards it, falling down in a state of utter exhaustion at short intervals. She was kindly cared for till her death, nearly two weeks after, by the Right Rev. Bishop Tozer, and her story was to the effect that when the dhow was skirting the beach before coming into the harbour, she and two others were thrown overboard, and that the water being shallow she managed to wade to the shore without being drowned, but she

thought that the others were dead. Such cases are not exceptions, but the rule in the conveyance of slaves. While the woman was alive, the case was reported to the English consulate, the circumstances stated and the brand to identify the slave was given, bnt nothing whatever was done by the Government of H.H. the Sultan to investigate the case and disprove the woman's statement, although the case could have been traced out with the greatest ease; the real fact being that the murder of slaves is no crime, any more than the wanton crushing of a worm. Slaves after landing are as a general rule not unkindly treated in Zanzibar, but that such scenes as occur in slave dhows during the course of their transmission should be allowed is a disgrace to civilization. The horrible sufferings to which these miserable wretches are wantonly exposed is unsurpassed, indeed it could not be surpassed, and this takes place as a matter of course in open daylight without even an attempt to screen it, even with the sanction of a Government having treaties with European nations and in fact supported by them. Slave gangs are driven along the streets in broad daylight, reduced to a skeleton state by starvation and disease, men, women and children, none having more than a small piece of coarse bagging or some twisted leaves to cover their nakedness, and many as naked as when they were born, not children, but grown-up men and women without even the primitive "fig leaf." No estimate could be given of the ravages of the epidemic amongst these the most miserable of all God's creatures since man was created.

During these dreadful months the natives of India, who keep an exact record of all their deaths, lost 6½ per cent. of their people. The mortality amongst the Arabs and other races between them and the slave population could not be less than 10 per cent.; the death rate amongst the negroes reached at the very lowest 25 per cent.; and that amongst the floating population was at least as high. The mortality in the town and suburbs I estimate at 15,000, and throughout the island generally 25,000. Connected with the shipping nineteen Europeans and Americans died. No Europeans or Americans permanently resident on the island died and only two were attacked, although the number at the time of the epidemic was about sixty.

Regarding the sanitary state of the town, it can only be said that it is filthy in the extreme, the only clean quarter being that occupied by the Europeans. Nothing beyond this and what has been already referred to can account for

the violence of the epidemic in December, unless it be that it was the month of Ramadan, when all the Mahomedans fast from before sunrise till sunset.

The fast was generally observed, and by the Bahoras in particular, with the greatest strictness. They did not suffer in consequence; but I observed among the larger population of the Khojahs, that many were seized with the first symptoms after the full evening meal. In the great majority of the cases among the natives of India, the seizure was in the early morning between three and six o'clock.

Time does not permit me to consider the epidemic further at present, and as notes regarding symptoms, treatment, &c., may not be within the scope of the "Epidemiological Society," I reserve them for future consideration.

P.S. I was kindly permitted by Dr. Kirk, H.M. acting consul at Zanzibar, to extract what information I could from the archives at the consulate. Dr. Kirk, in his reports to Government, states that the first epidemic occurred in 1840-45, but on showing him my data for fixing it at 1835-36, he agreed that it was the proper date, that given to him being stated in general terms without any means of correction or proof, and I was requested by him to notice this in my report.

N.B. On page 271 it is stated that reports had reached Zanzibar that cholera had broken out and spread in the Uniamezi country. As this report may reach Europe from other sources I beg to contradict it. The latest letters received from Uniamezi make no mention of cholera being there or at any point to the west, but it is undoubtedly still present on the Uniamezi caravan route, and caravans proceeding from that country towards Zanzibar have been attacked. My Arab informant stated to me yesterday that a caravan in which he is interested was not many days ago attacked by the disease at three days' march from the coast at Bagamoyo; and reports confirmatory have come from a Roman Catholic missionary at present in the district, that in travelling towards the interior he had to deflect his course owing to the number of putrid bodies lying near the path. The mortality was confined to the pagazi or porters.

In so far as we know, the region where Dr. Livingstone is supposed to be is free from the epidemic, but there will be great difficulty in forwarding supplies through the infected regions. J. C.

Zanzibar, 20th August, 1870.

ADDITIONAL NOTES ON THE CHOLERA EPIDEMICS ON THE EAST COAST OF AFRICA.

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[*Second Paper. Read February 14th,* 1872.]

First Epidemic.—I regret much that I have to correct an error in the date of this epidemic. I state that it occurred in December, 1835, and January, 1836. The correct date is December, 1836, and January, 1837. The mistake occurred in consequence of the difference between the Mahommedan and Christian year. Regarding this epidemic, I have nothing further to communicate.

Second Epidemic.—In the date of this epidemic there is also a mistake. Instead of 1859-60, it ought to be 1858-59. That this date is correct is made certain by the statement of Captain Burton, in his *Lake Regions of Central Africa,* vol. ii, p. 319. "In the early part of 1859, a violent attack of cholera, which extended from Maskat along the eastern coast of Arabia and Africa, committed terrible ravages in the island of Zanzibar and throughout the maritime regions." At the time of writing, I was not aware of the origin of the epidemic, and I was led this year to doubt the correctness of the date, from having been informed that cholera was prevalent in Arabia in 1858. Arabs from Makallah informed me that it was prevalent there, and that it reached their district from Jiddah. The American barque *Maryland* left Muscat on the 22nd October, 1858, and arrived at Zanzibar on the 15th November of the same year, and brought the news that cholera was prevalent in Muscat on the departure of the vessel. The epidemic reached Africa from the Arabian coast, and the first case on the island of Zanzibar occurred at the end of November, 1858, a negro in the ser-

vice of Ludda Damji, manager of the Customs, having been attacked. The epidemic ceased at the beginning of March. It is somewhat remarkable that during this epidemic, as in the last, the fatal cases among the Europeans were confined to the shipping two deaths having occurred in February, on board the American whaler *Zantho*, then in the harbour.

Third Epidemic.—The date of this epidemic is correct. In the course of investigating the origin of the second epidemic, I ascertained that, in 1865, there was cholera at Mecca and in Yemen, but that it did not reach the district of Makallah. My informant, an Arab of remarkable intelligence and of high standing, was in the infected region at the time, and he stated that it originated or rather proceeded from the Red Sea ports, and that it was conveyed along the coast in dhows. I thought that this was sufficient to connect the epidemic in Africa with that in Arabia by means of dhow communication; but on interrogating the people from Mukdesha, I was surprised to hear that, instead of reaching that place direct from Arabia or the Red Sea, it came from Brava and Merka to the south. The evidence of these people could not be doubted, as they were positive on the subject. At this time I had the good fortune to meet Mr. Brenner, at present resident in Zanzibar, but who was, in 1865, in connection with the exploring expedition of the late Baron Von der Decken, which left Zanzibar in May, 1865. The party, after visiting several places on the coast and making short excursions towards the interior, entered the River Jub in June. During the interval they encountered the epidemic, which was raging with great violence in the coast towns. Several natives in connection with the expedition died, and the Baron himself had a very severe attack, but recovered. When the party reached the town of Bardesah on the Jub, soon afterwards the scene of the brutal murder of the Baron and the medical officer of the expedition, they ascertained that the country in that neighbourhood was at the time free of cholera, but that the district had been affected. On inquiring concerning its line of progress, Mr. Brenner was informed that it came to Barderah from Gananah, along the trade route, and to Gananah from Berbera, on the Gulf of Aden, along the great caravan route. From Barderah, on the Jub, it passed towards the coast to Brava, and from thence northward to Merka and Mukdesha, and to the south, as stated in my former paper. The epidemic reached the coast in May, and continued till August, and spread from the coast towns

toward the interior, but not to any great distance. I had previously looked upon Berbera as a locality where a cholera epidemic was very likely to originate, or from which it would spread over Africa. At Berbera there is a great annual fair held, which lasts from October till April, the place being entirely deserted during the interval. It may be described as the meeting-place for the various inland tribes and the traders from the Red Sea, the Persian Gulf, the Gulf of Oman, Aden, and various parts of India. Formerly as many as twenty thousand strangers were usually collected there at one time: and, although the fair has decreased much in late years, still there is annually a very large gathering. The inland tribes from the neighbourhood of Gananah would probably leave Berbera in the month of March on their return journey. I am not certain that cholera actually prevailed as an epidemic during the time of the fair, but there can be no doubt regarding its line of progress, or regarding its being at about that time in the Red Sea ports.

Fourth or Present Epidemic.—I had an opportunity of examining lately two men who were leaders of the caravan which penetrated the Masai country, and brought to the coast the first intelligence of the existence of the epidemic in the interior. Their narrative was one of great interest in many respects, and they seemed to be thoroughly acquainted with the territories described. One had made fifteen caravan journeys amongst the Masai and the other nine.

The caravan left the Pangani in November, 1868, with over a thousand men, and proceeded towards the interior, keeping the river Pangani, or Rufu, as it is called, higher up, on the left. On the fourteenth day of marching they reached the Lake Jipe, and on the sixteenth day the town Urombo, near Mount Kilimanjaro, and on the twentieth, Marago ya Kanga the first Masai town. On the forty-ninth day they reached the Masai encampment at Kisima in the district of Laikepya, in the fifth month after their departure from the coast. There are many delays in travelling, and sometimes they remain for more than a week in one place without moving. When at Kisima they were told that the Masai had made a raid on the Somali to the north, and that while in their country they were attacked with some deadly disease which they brought with them to their own country, but they had no name for it, and had never known it before. At Kisima, the party which had reached the district separ-

ated in all directions to trade with the Masai, and, after being absent for about a month, they reunited and started on their return journey. One of their party died at Kisima, but they did not suspect that it was cholera, as it did not spread. On their return they made a detour, and kept the Kilimanjaro to their left. On the seventh month they reached the town of Lytoketoke, not far from Kilimanjaro, and there they saw for the first time people dying of the plague or cholera. Formerly it was merely a matter of report, but, when they saw those affected, they knew at once the nature of the disease, and hastened towards the coast. Lytoketoke is six days from Harush, and when they reached that place they met a large caravan from the Pangani going towards the Masai country. They told them that the whole district was affected with cholera, and advised them to return; but, not attending to their advice, they pushed on, but altered their line, not passing in that of the returning caravan which reached the coast in safety, only one of their number having died. I have every reason to believe that the statement of these men is correct, as I had several opportunities of checking their distances. The town of Kisima must be considerably to the north of the equator. They said that they never went into the country of the Somali or Gallas, but that they had met traders from Brava who regularly trade in these districts. Their description of the countries through which they passed and the rivers crossed, was of great interest, but foreign to the present inquiry.

Another man, one who was with the caravan met on its inward journey, was examined previously, but not by myself personally. I regret this, as he was one of the few survivors who reached the coast, but I hope to be able hereafter to get hold of him.

He stated that, on the fifteenth day from Pangani, they arrived at Harush, where a portion remained to trade in the district; that the remainder passed on to Dasikĕra, where another portion remained, and that, as ivory was very dear and scarce, the remainder pushed on to the district of Laikepya, where the great body of the Masai were. When there, they knew that the Masai were dying in great numbers, and as those of the expedition were also attacked, they collected their ivory and goods and, having concealed the greater part in a hole which they dug, fled to the coast. On their return, they passed through the districts of Dasikĕra and Harush, but the whole of the trading party in these two

places died. After their arrival at the coast, cholera appeared, and it appeared in all the districts through which they passed. Out of about one hundred and fifty of the party that started from the frontier of the Masai district only seven reached the coast alive. This man stated that when in Laikepya he heard that the Masai had made an attack on the Somal Gurra tribe, who possess camels, horses, and cattle, and that they had brought the disease to their own country. He also said that the next tribe to the Somal Gurra was the Galla Borani, whose territory is on the south of the Jub River. This brings us very near to the line of the cholera epidemic of 1865, stretching from Berbera to Gananah and Barderah, but beyond the district of the Somal Gurra I have not been able to trace it. Investigations, however, from the direction of Brava have been set on foot, as trading parties from that place visit the Somal Gurra, the distance being under a month. If the disease did not start from Berbera it probably proceeded from Abyssinia, which is very likely; or from some place on the west coast, crossing at about Gondokoro. But conjecture is useless. I have requested several people about to start shortly for the Masai country to collect further information both as to its origin and also regarding its devastations among the people.

On the arrival of the natives from the northern ports, I collected information regarding the spread of the epidemic to the north. It passed along the west generally, attacking the inhabitants of all the towns in communication with Zanzibar. When direct communication with Lamoo was open on the change of the monsoon, the epidemic reappeared with greater violence than formerly, when it first reached that place from Melinda. It lasted four months, and the only section of the population not attacked was the natives of India. It also appeared at Mukdesha and Worsheikh, and affected the population of the maritime districts to a distance of from two to three or four days from the coast. It appears reasonable to suppose that it did not pass further inland in the districts to the north, simply because at that season the trade routes were closed, and the people residing in one district belonging to distinct tribes, have no intercourse with their neighbours. The people from the inland tribes generally bring their produce to Brava, Merka, Mukdesha and other places before the dhows leave for Zanzibar, and they return with their purchases at once. The produce that they bring consists of hides, ivory and gums such as myrrh and arabic, but principally the two

former; orchella weed and gum copal being a coast produce. The ivory and slave caravans starting from between Quiloa and the Pangani go at all seasons, and consequently those starting from infected districts carried the disease with them along their line of march. This epidemic reached to the north as far as the island of Socotra. The Socotra dhows leave Zanzibar very early, at the first break of the monsoon, and generally call at Mombassa, the whole passage being about fifteen days. My Socotra friends state that in one of their dhows one man died on the passage and that on arrival at its destination, the disease broke out at Bunder Kadu near to the principal town, Bunder Kalassia. It did not spread generally throughout the island and there were only about twenty deaths at Bunder Kadu. They spoke of a former epidemic which was very fatal at Bunder Kalassia, and which I supposed to be that of 1858-59.

The time has not yet arrived for the collection of full particulars regarding the spread of the epidemic to the south of Zanzibar, so that the remarks to be made are incomplete and some may require correction. The disease lingered in Mozambique and the neighbourhood for about eight months. On Januury 11th, 1871, it appeared at Ibo, to the north of Mozambique, and at about the same time at Quilimane to the south, being conveyed in both cases in dhows from the infected Mozambique district.

My informant, the principal banyan merchant at Ibo, states positively that cholera did not reach that place by coast traffic but by dhows. It lasted for a month and a half and there were about thirteen hundred deaths, but no natives of India were attacked. Five of the higher class of Portuguese died and twelve Portuguese soldiers. At the time of his departure from Ibo, about the beginning of May, no cases had occurred for about a month, the last case that he heard of being on the 11th of March, and he had heard that the epidemic had passed towards the interior to about five days' journey from the coast. The last case of cholera at Mozambique was in March last.

Native merchants from Nosi-bé inform me that cholera appeared at Majunga on the north-west coast of Madagascar in August, 1870, and that it was conveyed in native craft from Mozambique. It was communicated from Majunga to the French island Nossi-bé in September. The disease was then confined to the native town Ambanourou, around which a *cordon militaire* was placed. The epidemic did not spread at the time. During this epidemic there were

about one hundred and sixty deaths. In March, 1871, cholera again appeared at Nossi-bé at the time of the arrival of the Seychelles mail and three men on board the mail steamer died. I have heard nothing regarding the mode of communication. The disease was confined entirely to the French town of Helleville; not many deaths had occurred, and a cordon was placed round the town.

In December, 1870, a quarantine of fourteen days, afterwards of twenty-one days, was established at Mauritius, as the north coast of Madagascar was infected and as cases supposed to be sporadic had occurred in Mauritius. In January, 1871, it is reported from the same island that several cases had occurred, usually fatal, and fearing that the malady had been conveyed from Madagascar, a petition had been presented to Government, praying that communication between Madagascar and Mauritius may be "entirely prohibited" until the latter island may be free of cholera.

[In Dr. Christie's first paper a mistake occurred, which has been corrected in the *Transactions*, but which is to be found in the report of the paper as it appeared iu the *Lancet*. In the original paper H.M.S. *Cossack* was erroneously written in place of H.M.S. *Dryad*. See p. 277].

OBSERVATIONS ON OUTBREAKS OF CHOLERA IN SHIPS AT SEA.

By ROBERT LAWSON, Esq.,

INSPECTOR-GENERAL OF HOSPITALS, ROYAL ARMY.

[*Read June 10th*, 1871.]

At the solicitation of Dr. Milroy I have arranged the memoranda in my possession regarding outbreaks of cholera in ships at sea for submission to this Society. In addition to the interest these narratives excite, some conclusions may be drawn from them of the utmost importance in the present state of epidemiology. In investigating such points we have frequently to deal with slight manifestations of disease, which, in the default of a knowledge of their causation, many attribute to chance; but Nature does nothing by chance, and never presents us with a result, however trivial it may appear, which she has not brought about by the action of a sufficient cause; and no one can succeed in explaining her laws who neglects even the slightest indications she presents of their operation.

Most of those present are aware that I have supported the view that the concurrent action of at least three classes of causes is necessary for the development of epidemics—viz.: (1) General causes, one of which I have indicated, in connexion with fever and cholera, by the term Pandemic Wave; (2) those connected with locality, as distinguished from the persons who reside in it; (3) causes connected with persons, as distinguished from those depending on locality. If the latter classes be fully developed, intense disease may be excited under the influence of the general causes; while, if they be less developed, notwithstanding the operation of the general causes, sporadic cases or small groups only may make their appearance, or the inhabitants of certain localities may escape altogether, while those of others in their vicinity (and often

mixed up with them in the most extraordinary manner) may display a large amount of sickness.

When examined for a sufficient length of time, and over an adequate space, the course of Pandemic Waves is found to be uniformly from south to north according to a definite law, and their position from year to year can be indicated on a map by lines of equal magnetic dip. This has been done in the map on the wall* for January 1st of each consecutive year. These lines, it will be seen, in some places pursue nearly the same direction as the parallels of latitude; in others, as down the western shores of Europe and Africa, they cross the parallels of latitude at considerable angles. The whole system of isoclinals has a motion to the westward, each portion, however, maintaining nearly the same angle with the parallels of latitude as represented on the map. From this it follows that where the isoclinals pursue nearly the same course as the parallels of latitude, they present little change of latitude on any given meridian for a considerable number of years; but where they are more oblique, as on the west coast of Europe and Africa, their motion to the northward, on any meridian, is so rapid that in a few years it becomes sensible, and requires to be allowed for. This change at London is equal to $1\frac{1}{4}$ days per annum in the arrival of the wave, and the same may be employed as far as St. Helena; at the Cape the change amounts to $1\frac{2}{3}$ days per annum. As the isoclinals in the map are their positions in 1840, the corrections named must be applied for the number of years which may have elapsed in any particular case within the limits mentioned. The position of the advancing edge of the wave for any period of the year 1840 may be found by simple proportion from the lines on the map, and this must be corrected in the manner indicated for subsequent years.

It is obvious that if there be a series of waves proceeding from south to north, which can influence the frequency of cholera in communities otherwise favourably circumstanced for its manifestation, we should find, from time to time, that ships passing in the opposite direction experienced one or more outbreaks of cholera in the positions where they run into these waves, and that there should be intervals between them, of greater or less extent, comparatively free from the disease; the same should be experienced when they go from

* See also map in Sanitary Report for the Army for 1866, p. 383, and Epidemiological *Transactions*, vol. iii, p. 216.

south to north, provided only they proceed more rapidly than the wave; while, if their course be nearly east or west, they may continue from commencement to end of voyage in the same waves. Having premised these explanations, the details of various outbreaks may now be given, and it will be seen how far they support this view.

The first case to which I have to draw your attention is that of H.M.'s ship *Apollo*, which carried the 59th Regiment from Cork to Hong-Kong in 1849. The first account of the occurrences in this ship, to which I had access, was that by Dr. Bryson, late Director-General of the Naval Medical Department, in a small volume "On the Infectious Origin and Propagation of Cholera," published in 1851. I have recently been so fortunate as to meet the medical officer who was in charge of the troops in the *Apollo*—Dr. Fraser, 10th Hussars—and have been favoured with the perusal of his journal and reports, which afford many details not given in Dr. Bryson's account.

The *Apollo* was a frigate-built ship, having beneath the upper or weather deck a gun deck with ports on each side, and below this the orlop, which had small side scuttles, which could be opened in fine weather only. The gun deck was occupied by the officers abaft, and by the crew on the starboard side forward, and a portion of the troops on the port side; the remainder were on the orlop. The latter, of course, could not be so efficiently ventilated as the former. The troops, including women and children, amounting to 593 persons, embarked on June 12th, and the ship sailed on the 17th. Cholera had prevailed in Cork and its vicinity previous to the troops going on board, but there had been none in the ship herself up to this time. Subsequently the course of the disease was as follows: (See opposite page.)

The first case of cholera occurred in a soldier who had had diarrhœa for some days, who on the morning of June 18th went on deck, after getting out of his hammock much heated, and placed himself under a water-tap, and let the water run over his body. Collapse ensued immediately; there were slight cramps with the characteristic evacuations, and he died in seven hours. No other case presented itself until nine days afterwards, from which they continued to crop up until July 10th. On the 14th and 15th there was heavy rain, and another case occurred on the 16th; the 17th was also rainy, followed by a case on the 18th. From the 19th to the 22nd inclusive the ship seems to have been becalmed, as her position altered but a few miles daily, and

Date, 1849.	Ship's position at noon. Latitude.	Ship's position at noon. Longitude.	Winds. Direction.	Winds. Force.	Temperature.	Weather.	Cholera. Attacked	Cholera. Of whom died.	Remarks.
June 18 ...	51° 7′ N.	8° 34′ W.	S.S.W.	Slight	63°	Fine	1	1	
„ 27 ...	35 21 „	15 25 „	N.E.	Moderate	67	„	1	...	
„ 29 ...	At Ma	deira	...	...	70	„	1	1	A woman [not given
„ 30 ...	30° 14′ N.	16° 59′ W.	N.E. by E.	...	69	„	...	1	A child died; date of attack
July 1 ...	At Tene	riffe	...	...	72	„	...	1	„ „ „
„ 2 ...	Santa Cruz	12 miles	N.N.E.	...	72	Sailed 8 A.M.	1	...	
„ 5 ...	23° 58′ N.	20° 32′ W.	N.E.	Light	72	Fine	1	...	
„ 7 ...	20 35 „	25 21 „	E.S.E.	...	76	„ [at noon	1	...	
„ 8 ...	18 8 „	26 44 „	E.	...	77	Hot and close; slight rain	1	...	
„ 9 ...	16 36 „	27 38 „	E.	Fresh	75	Fine	1	...	
„ 10 ...	14 12 „	27 18 „	E.S.E.	„	79	„	1	...	A woman
„ 14 ...	7 25 „	25 37 „	S.S.E.	„	78	Very wet	...	...	
„ 15 ...	7 12 „	23 32 „	S. by W.	Light	80	Rain	...	...	
„ 16 ...	6 20 „	23 35 „	S.	...	81	...	1	1	
„ 17 ...	5 14 „	24 3 „	S.S.E.	Fresh	79	Heavy rain	...	...	
„ 18 ...	3 25 „	25 25 „	S. by E.	...	80	...	1	1	
„ 19 ...	1 58 „	27 31 „	S. by E.	...	80	...	5*	3	* Including 3 of the crew
„ 20 ...	2 8 „	27 14 „	S. by W.	...	80	...	5†	3	† Including 3 of the crew
„ 21 ...	2 6 „	27 24 „	S.	...	80	...	1‡	...	‡ A marine
„ 22 ...	1 50 „	27 32 „	S.	Light	80	...	1	...	
„ 23 ...	0 51 „	29 6 „	S. by W.	...	79	... [trade wind	2§	...	§ Including 1 of the crew
„ 24 ...	0 26 S.	30 50 „	S.S.E.	Fresh	79	Commencement of S.E.	...	...	
„ 29 ...	8 34 „	34 25 „	S.E. by E.	„	80	Showery	2	2	
„ 30 ...	10 24 „	35 5 „	S.E. by E.	...	78	Showery and squally, A.M.	3	2	
Aug. 4 ...	21 56 „	39 41 „	N.N.E.	...	76	Lost S.E. trade.	...	...	
„ 6 ...	23 6 „	42 48 „	S.E.	Very slight	76	...	1	1	
„ 12 ...	Off Estre	lla Bay.	S.W.	...	73	Cloudy, hot, and close	1	1	

during that period there were twelve attacks, including the quarter-master of the 59th (the only officer attacked) and seven of the crew (the first among them), and six deaths. On the 23rd there were two attacks, including one of the crew (the last of these affected). The ship seems to have got a breeze on the afternoon of the 22nd, and in the course of the 23rd to have met the S.E. trade, and for some days there was another lull in the attacks, but on the 29th and 30th, while in the S.E. trade, there were five attacks and four deaths, and on August 6th and 12th another two, after she had passed out of the trade altogether.

During the prevalence of the malignant disease Dr. Fraser states diarrhœa and colic prevailed to an unusual extent, and appeared to keep pace with it, increasing as it increased, and disappearing generally with its disappearance. The diarrhœa was generally of such an aggravated form that he thought many of the cases, if not most, would have been returned during an epidemic in England as true Asiatic cholera, presenting as they did profuse vomiting and purging, great prostration, remarkable sinking of the features, and even slight cramps occasionally; but he carefully refrained from designating any case cholera in which the characteristic rice-water evacuations were not present, and the natural secretions suppressed.

The appearance of the men soon after embarkation attracted the attention of the officers on board, both naval and military. Dr. Fraser describes them as attenuated and without spirit, their eyes sunk in their heads. They were on two-thirds allowance, as was customary at that time. Preserved meat was served out every fourth day; the contents of many of the cases were found putrid, and great quantities of it were thrown overboard on every occasion on which it was issued. The men had an excessive dislike to the preserved meat, and were fully persuaded it was the chief cause of the disease among them. They also had a strong aversion to the cocoa issued for breakfast, and, on representation, tea was substituted for that, the issue of preserved meat stopped, and oatmeal-gruel given in the evening, from about the middle of July to September 7th.

Whether the issue of preserved meat to the crew was suspended during this period is not mentioned, but Dr. Fraser has since informed me he thinks it was not.

It will be remarked that, though cholera had been in the ship for a month previously, none of the crew were attacked before July 19th. There were two wide tubes on each side

for allowing heated air to escape from the orlop, but as they terminated on the gun deck, instead of being carried into the open air, they merely relieved the lower deck of impure air by diffusing it on that above. Dr. Bryson states that these tubes were not opened until July 17th, two days before the occurrence of the first cases among the crew, and he adds, "the greater number of the cases on this deck occurred among the men belonging to the messes close to the apertures of the tubes, or to the main hatchway, by which the impure air also escaped;" and he concludes that the disease was communicated from the lower deck to that above by means of this air. Dr. Fraser had made a diagram showing the relative position of the various messes, and of these tubes and the hatchways; and those, members of which were attacked, were also indicated. From this it appears the messes were numbered from 1 to 8, No. 1 being the foremost, No. 8 the aftermost. There was a gun between 1 and 2, another between 2 and 3, and a third between 3 and 4. One of the tubes was between the second gun and the third mess, the other between Nos. 4 and 5 messes, while Nos. 4 and 8 messes were about equidistant from the main hatchway, and 5, 6, and 7, of course, nearer it; No. 2 mess also was directly abreast the fore hatchway. Now, the cases presented themselves as follow:—

In No. 1 mess, away from hatchway and tubes—one case on 19th, one on 23rd.

In No. 3 mess, beside a tube, but away from a hatchway —one case on 19th, one on 20th, one on 21st.

In No. 8 mess, in vicinity of main hatchway, away from a tube—one case on 19th, two on 20th.

So that No. 2, near a hatchway, Nos. 4 and 5, with a tube between them, and as near the main hatchway as 8, and Nos. 6 and 7, still nearer it, altogether escaped. It is obvious, therefore, that mere vicinity to the outlets from the lower deck, and presumably to the emanations from it, could not have been the active cause of these attacks among the crew, though, no doubt, during the close, calm weather existing from the 19th to the 22nd, the vitiated air from the lower deck would be less rapidly diffused, and, so far, more injurious, than either before or after, when there was a good breeze. If, as Dr. Fraser thinks, the crew continued to use the preserved meat, it is possible that some more tainted than the rest was consumed by the messes which suffered.

The authorities at Rio Janeiro would not allow the *Apollo*

to communicate with the shore, but directed her to proceed to the Ilha Grande, about sixty miles to the westward, where her crew and passengers were disembarked, and the holds cleared out. These were found clean, dry, and free from offensive effluvia, and none of the persons engaged in the work were attacked, nor, indeed, did any more cases of cholera occur during the remainder of the voyage. After leaving the Ilha Grande, on September 7th, the *Apollo* called at the Cape, where she seems to have taken in water, at least; subsequently she touched at Booroo, where more water was taken on board, and immediately after its employment was commenced bowel complaints became frequent, and there were more admissions from this form of disease in January than in the four previous months. On February 1st the use of the Booroo water was discontinued, and some which had been obtained at the Cape substituted, and the number of cases of diarrhœa at once diminished. Though not connected with the cholera in the *Apollo*, this fact in the narrative of her voyage was too significant to be omitted.

The next case to which I will direct the attention of the Society was that of the *Renown*, which took the 1st Battalion 9th Regiment from Gibraltar to the Cape in 1865. The *Renown*, a fine ship of 1293 tons, with a crew of fifty-two persons, sailed from Kingston for Gibraltar, with troops on board, on August 3rd, 1865, and arrived on 17th, and there does not seem to have been any bowel complaint among them or the crew during the voyage. The head-quarters 1st Battalion 9th Regiment, including sixteen officers, 353 men, twenty-eight women, and sixty-five children, embarked in this vessel on August 21st, part at 6.15 a.m., and the remainder at 5 p.m. The men were in five companies, designated A, B, D, F, and K. There was a single berth deck extending the whole length of the vessel, which was extremely well ventilated with side scuttles, stern lights, and shafts leading to the open air above the upper deck. The hospital, as is now the practice, occupied a part of the port side abreast the main hatchway. The companies were berthed as follows:—On the starboard side, forward, was A, followed by F, which occupied the space abreast the main hatchway, and farther aft were the band and drums. On the port side, forward, was B Company, followed by K, which occupied the space up to the hospital bulkhead; D Company was in the centre of the vessel in front of the main hatchway, between K and part of F. The women and children were in the after part of the deck.

On the morning of August 22nd, a man of F Company, who had embarked on the morning of the 21st, and been employed during the day in assisting to get the baggage on board, was attacked with cholera; he was landed immediately, and died in a few hours. Though the disease had been at Gibraltar for a month previously, no case had occurred in the 9th Regiment, nor in the barracks from which they came, up to the time of their embarkation, and as no fresh case occurred up to the evening of the 23rd the ship then sailed for the Cape, and everything went on well for several days. On the 29th a young child had an attack of diarrhœa, from which it recovered by the 31st. On the 30th another child was affected with diarrhœa, but was well by September 3rd. From this date the attacks became more numerous, as indicated in the following table:—

Date, 1865.	Ship's position at noon.		Winds.		Temperature.	Diarrhœa, Dysentery,		Cholera.	
	Latitude.	Longitude	Direction.	Force.		Admitted	Died.	Admitted	Died.
Sept. 1	22° 1′ N.	24° 35′ W.	N.E.	Slight	78°	...	...	...	...
„ 2	19 52 „	25 38 „	„	Moderate	79	...	...	...	...
„ 3	17 35 „	26 48 „	„	Light	80	3	...	...	...
„ 4	14 51 „	27 8 „	N.N.E.	Moderate	81	...	...	...	...
„ 5	12 16 „	27 16 „	S.E.	Light	82	...	...	2	1
„ 6	11 9 „	27 2 „	E.S.E.	„	83	1	...	...	...
„ 7	10 19 „	27 14 „	„	„	83	...	...	...	...
„ 8	8 51 „	26 12 „	W.S.W.	Moderate	81	1	...	...	...
„ 9	7 41 „	24 18 „	S.W.	„	81	...	...	...	...
„ 10	6 27 „	21 40 „	S.S.W.	Light	80	1	1	2	1
„ 11	5 11 „	20 46 „	Southerly	„	81	...	...	5	5
„ 12	3 51 „	25 17 „	„	„	80	3	...	1	1
„ 13	2 31 „	27 5 „	S.S.E.	„	79	...	...	2	...
„ 14	0 56 „	28 56 „	„	Moderate	80	2	...	...	...
„ 15	1 23 S.	30 8 „	S.E.	„	80	5	...	...	...
„ 16	4 33 „	31 11 „	„	„	79	4	2	3	3
„ 17	7 50 „	31 59 „	E.S.E.	„	80	1	...	...	...
„ 18	10 57 „	30 50 „	„	„	80	1	...	...	...
„ 19	13 58 „	30 04 „	„	„	78½	3	...	2	2
„ 20	16 22 „	29 34 „	„	Light	79	2	...	...	...

Subsequent to September 20th no one was attacked with bowel complaint or cholera, and there was no death after that date. The ship reached the Cape on October 9th.

The provisions on board the *Renown* were all of excellent quality. A quantity of water had been taken on board at London, and some at Gibraltar, which was stowed partly in casks and partly in tanks; and though that in the casks was somewhat coloured by them, yet it was considered

good. There was a distilling apparatus on board capable of making 500 gallons of fresh water daily, and, from the time of the appearance of the cholera, water from this only was employed for cooking and drinking. The cargo consisted of a general assortment of goods for the Indian market, with iron, and shingle mixed with sand, for ballast; under and around the main hatchway there was a quantity of the latter, which, when I was on board the ship in Algoa Bay in November, was damp, but did not present any trace of mud mixed with the sand. Unpleasant odours from the hold were not complained of during the voyage. After cholera appeared the people were kept as much as possible on the upper deck during the day, and every means employed for ventilating the berth deck as freely as possible, and, as the weather was fine throughout, all the ventilating apertures were constantly open. But for the care taken with these points many more cases might have occurred.

As regards the immediate cause of the cases in the *Renown* the following facts are material:—It will be remembered that a man of F Company, which was berthed alongside the main hatchway, was attacked the morning after he embarked, and sent on shore at once. The next cases were those of another man of the same company and of a child who was accommodated in the women's berth at one side, but abreast a small scuttle which led down to the after hold, which was occasionally open. This was the child of a sergeant of F Company, who, with his wife and another child, had been attacked with dysentery on September 3. The attacks and deaths occurred in the different companies, and among the women, children, and crew, as below:—

Company, etc.	Attacked. Dysentery and Diarrhœa.	Attacked. Cholera.	Died. Dysentery and Diarrhœa.	Died. Cholera.
A Company ...	—	—	—	—
B " ...	1	1	—	1
D " ...	4	—	—	—
F " ...	4	10*	—	7*
K " ...	3	2	—	2
Women ...	3	1	—	1
Children ...	12	2	3	2
Crew	—	2	—	2

Bearing in mind the position of the different companies, and of the women and children in the ship, and the fact that the first case in the regiment and ship was a man of F Company, which subsequently suffered most, and that the others

* Including the first case at Gibraltar.

were affected nearly in proportion to their proximity to the hatchways, it is pretty clear that emanations from the hold were instrumental in developing the disease. The crew were all berthed in the poop or forecastle, which did not communicate with the hold. Of these, the surgeon, a man of broken-down constitution, and one seaman, whose duties required him to be frequently near the hatchway, or in the hold, were the only persons attacked with cholera, both of whom died. To complete the history it may be added that the men sick of cholera were treated in the hospital, the women and children in the women's berth. The bodies of those who died were removed on deck at once, and buried soon after, and their bedding and clothing thrown overboard. The bedding and clothing of those who recovered were freely exposed to the air for some time before being used again.

The *Renown* was placed in quarantine on her arrival at Cape Town, and a few days after left for Saldana Bay, where the troops were disembarked, and the vessel thoroughly fumigated and whitewashed, and the bedding used by the troops destroyed. Towards the end of October she proceeded to Algoa Bay, where she arrived on November 1st, still in quarantine, and anchored three miles off Port Elizabeth. There had been no case of cholera or bowel complaint among the crew since the troops disembarked. On November 8th she was admitted to pratique, and on the 9th the head-quarters of the 96th Regiment embarked, and the vessel sailed the same afternoon for Bombay. On November 12th one of the 96th, who had had slight diarrhœa for some days previous to embarkation, was attacked with vomiting and purging, the evacuations being like rice-water, but being seen to immediately, collapse did not ensue. The man slept on the port side, near one of the hatchways, a place clean and well ventilated. There had been one case of a similar nature, but with some collapse, on shore at Port Elizabeth on October 30th in a civilian.

In 1866 two ships left the Thames for India with troops on board, in both of which cholera appeared, and presented some features of interest. The details concerning them are not so complete as those given above, but what I have been able to obtain from the official reports of the medical officers, and from the copies of the logs at the office of the Registrar-General of Seamen, is given below.

The first of these, the *Windsor Castle*, was a fine roomy vessel of 1,074 tons, with a single troop deck, extending her

Date.	Ship's position at noon.		Remarks.
	Latitude.	Longitude.	
Sept. 25	...	...	One man attacked; recovered
„ 26	49° 29′ N.	5° 2′ W.	One man attacked and died
Oct. 8	36 15 „	16 59 „	One man died
„ 10	32 18 „	16 6 „	One man died
„ 15	23 11 „	18 16 „	One man died; one child died
„ 16	21 14 „	20 11 „	One child died [died
„ 17	19 15 „	20 36 „	One man died; one passenger (female)
„ 19	14 32 „	20 51 „	One passenger died
„ 24	7 58 „	20 53 „	One man died

In 1867 H.M.S. *Jumna*, one of the new Indian steam troop-ships, was on her passage outwards. She had a number of military officers on board, but no troops. On 17th July, four days after leaving St. Vincent, one of the Cape de Verde Islands, one case of malignant cholera and six of choleraic diarrhœa occurred among the crew, and the latter continued to present themselves until the 27th, by which time 46 had been entered on the sick-list, besides some 10 or 12 milder, which were not. The ship's position at noon on 17th was lat. 6° 35′ N., long. 15° 46′ W., and on 27th she was in lat. 19° 37′ S., long. 5° 27′ W. Some fresh beef, obtained in England thirteen days before, and brought out in the ice-house, was issued to the crew on the 17th; the marines, who used the beef, suffered more than the rest of the crew, while the warrant officers, who refused it, escaped. One of the military officers had a severe attack of choleraic diarrhœa. There was no cholera at St. Vincent while the *Jumna* was there. Besides the beef, it was supposed that the fresh bread made on board was sour, and its issue was stopped and biscuit substituted. These facts are principally from the Report on the Health of the Navy for 1867; the positions of the *Jumna* were communicated by Dr. Mackay.

The ship *Newcastle*, from India to England, with some detachments of troops on board, sailed from Cape Town for St. Helena on the evening of 2nd April, 1866. About 4 a.m. on the morning of the 8th a soldier was seized with vomiting and purging, the evacuations like rice-water, violent cramps, intense thirst, and soon became completely collapsed. He recovered in a few days. The ship arrived at St. Helena on the morning of the 12th April. There had not been any cholera in this ship since she left India on her home voyage.

There have been numerous outbreaks of cholera in vessels proceeding from Indian ports to the southward; but, unfortunately, the details as to the ships' positions and the dates

of attacks have not been given, and the full significance of these facts remains in abeyance. I have been able to collect a few, in which these particulars are available to some extent, which will permit of their being referred to here with advantage.

The first to be mentioned, noticed by Dr. Macpherson in his work on *The Cholera in its Home*, p. 25, was in the *Sultany*, which left Calcutta on February 10th, 1854, with a crew of 80 men and with 375 emigrants. They had no cholera going down the river; but a fortnight after leaving the Sandheads, or on February 29th, it broke out and thirty of the passengers died of it. The ship reached Mauritius on 24th March. Unfortunately, the ship's position on the appearance of the cholera is not given; but the resemblance of the case to others to be mentioned immediately deserves attention.

In the year 1859 the Peninsular and Oriental Company's steamship *Oriental* left Bombay for Mauritius with a portion of the 61st Regiment, which, including officers, men, women, and children, numbered 588. An outbreak of cholera occurred, which led to her touching at Galle, in Ceylon. The troops embarked on 29th June, and the vessel sailed at 2.20 p.m. the same day. The men were well provided for in every respect, save that they were rather crowded and without berths or hammocks, and a considerable portion were kept on the upper deck, where they had to sleep. They were supplied with new blankets of good quality. The first, and indeed most of the cases, occurred among those who slept on the upper deck, which was perfectly ventilated. At the time the *Oriental* sailed cholera was prevailing at Bombay. There is no account of the date of the attacks, but the deaths, with the ship's approximate latitude, occurred as follow:—

Jnue 30,	lat. about	17° N.,	1 man died from cholera at 9 a.m.	
July 1,	„	14½° „	1 soldier died from do.	7 a.m.
„ 2,	„	12° „	3 soldiers died from do.	
„ 3,	„	9½° „	1 woman died, said to be from sea sickness.	
„ 4,	„	7° „	2 soldiers died from cholera.	
„ 5,	anchored at Galle at 3.45 p.m., no death this day.			
„ 6,	at Galle		3 soldiers died of cholera.	
„ 11,	Do.		1 soldier died of cholera.	

The *Oriental* left Galle on 14th July, and arrived at Mauritius on 28th, no other case of cholera having presented itself.

The next case to which I will refer is that of the *Gertrude*, which left Calcutta for England in May, 1859. This case has been mentioned in Dr. Murray's paper on the treatment of cholera, published by order of the Government of India at Calcutta in 1869, Appendix II, p. 18. I have also seen the official report furnished by the military medical officer on board to the Director-General Army Medical Department, and have examined the official log of the ship deposited with the Registrar-General of Seamen. Each account differs from the others in some particulars. That given below has been drawn up so as to include every specific fact connected with cholera mentioned in any of them.

The *Gertrude* sailed from Calcutta on 21st May with 120 military invalids on board, besides some other passengers and her crew, and on the 27th got out to sea. A passenger died of disease of the heart on 9th June, in lat. 6° 44′ N., long. 87° 35′ E. Some cases of dysentery and diarrhœa seem to have occurred after sailing. The attacks of cholera, with the ship's position at the time of death, are given below; the others are estimated.

Date.	Ship's position at noon.		Remarks.
	Latitude.	Longitude.	
June 12	About 2° N.	About 89° E.	A boy of crew attacked
„ 15	...	...	Private Chester, 80th regiment, attacked; died on 16th
„ 16	4° 5′ S.	92° 16′ E.	Mr. Stephens, chief mate, attacked and died James Lister of crew died; cause not stated in log John Lacey, aged 6 years, Royal Artillery, attacked; died 17th
„ 20	About 11° S.	About 81½° E	Private R. Knight attacked; died 21st Private W. Stephenson, persisted he had cholera, passed into fever, and died on 25th Several others complained of choleraic symptoms, deemed imaginary, but they got well
„ 26	21° 57′ S.	65° 36′ E.	R. Turner, seaman, attacked; died 27th
„ 29	About 24½° S.	About 58° E.	Private Charles Fenton died of cholera

The next ship in point of time was the *Queen of the North*, of 840 tons, with a good, well-ventilated troop-deck. She had in cargo 340 tons lintseed in bags, and 150 tons bloodstone, with two feet of clean granite under these for ballast.

She made very little water, and the bilge was said not to have been offensive at any time; but occasionally a disagreeable odour from the hold was perceived. All along under the upper deck the timbers were exposed, which would allow emanations from the hold being diffused on the troop-deck. The provisions were good; but the water, which had been obtained at Bombay, was always observed to become opaline after exposure to the air for a little. On 21st January, 1864, 272 persons, consisting of officers, time-expired men, invalids, women, and children, embarked in this vessel at Bombay, and her crew numbered 27. She sailed for England the following day. Several of the invalids and women and children had suffered from bowel complaints previous to embarkation. One woman, while under treatment for diarrhœa, was attacked on the evening of the 24th with vomiting and purging, with a disposition to collapse, and died the following evening; but the medical officer in charge hesitated to call the case cholera. Two other cases, with collapse, but without cramps, and with bilious stools, occurred on the 28th and 29th January; both recovered. The first unequivocal case of cholera, in the surgeon's opinion, appeared on 1st February, and proved fatal the same day. The ship's position, with the state of the weather and the number of attacks each day while the outbreak lasted, were as follow:—

Date.	Ship's position at noon.		Winds and Weather.	Thermometer.	Attacks of cholera.
	Latitude.	Longitude.			
Feb. 1	2° 49′ N.	77° 7′ E.	N. to E., good breeze	81°	1
„ 2	0 47 „	77 43 „	„ slight	81	1
„ 3	0 43 S.	78 6 „	„ „	82	...
„ 4	1 30 „	78 21 „	Calm	84	...
„ 5	2 5 „	78 30 „	„	85	2
„ 6	3 0 „	79 5 „	Nearly calm	82	...
„ 7	4 2 „	79 45 „	Slight breeze, heavy thunder-storm	82	...
„ 8	5 38 „	80 36 „	Slight winds, squally	82	1
„ 9	5 54 „	81 18 „	Almost calm	82	3
„ 10	6 53 „	81 29 „	Westerly slight	82	6
„ 11	8 19 „	81 23 „	„ „	82	1
„ 12	9 16 „	81 13 „	Slight and variable	83	8
„ 13	10 4 „	81 2 „	Slight A.M.; in evening S. E. trade	83	10
„ 14	11 51 „	79 24 „	S.E.	83	4
„ 15	14 3 „	77 18 „	„	83	4

There was no attack after the 15th, and the last death was on the 19th. Altogether, from the 8th to the 15th,

there were 37 attacks, of which 24 proved fatal. Diarrhœa was very common, both among the passengers and crew, from about the 5th February; but I was unable to obtain the names or numbers of persons attacked. The cases of either disease did not come from any particular part of the troop-deck, but were diffused all over it. The crew, who lived in the forecastle, which had no communication with the hold, had no cholera; but the chief officer, who used to drink large quantities of water, was attacked and died.

The *Salamanca* had been fitted out at Bombay for the conveyance of a portion of the 89th Regiment from Kurrachee to England. She took with her from Bombay 1 officer, 43 non-commissioned officers and men, and after some weeks' delay at Kurrachee, received on board there 8 officers, 254 non-commissioned officers and men, 7 women, and 15 children, and went to sea on 7th May, 1865. Cholera had been prevalent among the native population at Kurrachee for some time, and a few cases had presented themselves among the troops. Care was taken, therefore, to see all the people embarked were free from it, and one sergeant having lately suffered from it was left behind with his family. There was, however, a case of sharp choleraic diarrhœa on 7th in a man who had embarked at Kurrachee and was suffering from sea sickness, and on 9th another, also suffering from sea sickness since embarkation, was attacked with cholera. The subsequent course of the disease is given below:—

Date.	Ship's position at noon.		Attacks of cholera.	Remarks.
	Latitude.	Longitude.		
May 9	20° 32′ N.	67° 59 E.	1	
„ 10	...	...	1	
„ 11	14 14 „	68 36 „	3	Several attacks of choleraic diarrhœa
„ 12	12 5 „	68 9 „	1	„ „ „ „
„ 13	About 10°	...	...	„ „ „ „
„ 14	8° 10′ N.	68 20 „	2	
„ 16	4 28 „	68 29 „	...	Heavy rain this day and on the 17th, and much intermittent afterwards

Six deaths occurred in all from cholera. The man attacked on the 9th was treated in the hospital. Immediately after death the body was stripped and placed in a boat to leeward until buried, and the clothes and bedding thrown overboard, the berth scraped and sprinkled with

Sir W. Burnett's solution (chloride of zinc). The subsequent cases, as well as those of diarrhœa, were treated on the forecastle, the evacuations thrown overboard as soon as passed, and chloride of zinc freely thrown down the latrines. The issue of porter was stopped on the 12th, and rum given instead. There was no attack of cholera after the 14th; but the frequency of intermittent subsequent to the rain of the 16th and 17th is worthy of remark.

The last instance I will bring forward is that of the *Durham*, from Calcutta to England, in 1866. She received 13 officers, 422 non-commissioned officers and men, 15 women, and 25 children at Calcutta on February 24th, and seems to have sailed the following day. She must have left the land on 26th or 27th, as her latitude on 1st March was 17° 20′ N. The ship's provisions were good, the ventilation sufficient, and the materials for fumigation ample. The first case of cholera appeared on 9th March; the course of the disease subsequently was as below:—

Date.	Ship's position at noon.		Attacks of cholera.	Remarks.
	Latitude.	Longitude.		
March 9	10° 15′ N.	90° 35′ E.	1	Five of these cases proved fatal.
„ 19	0 20 S.	88 15 „	1	There were three other attacks
„ 21	2 0 „	88 25 „	1	on board, but the dates are not
„ 22	3 10 „	89 30 „	1	given. While the cholera pre-
„ 25	6 35 „	88 20 „	1	vailed there were between 30 and
„ 29	13 10 „	82 40 „	1	40 cases of choleraic diarrhœa,
April 1	20 0 „	72 10 „	1	but the dates are not given.

The medical officer reported, "the intense heat between decks was so great that, at my suggestion, nearly half the men slept on deck. It is worthy of notice none of these men were attacked with cholera. I had frequent parades for the detection of diarrhœa amongst the troops. Salt pork as a ration and limejuice were temporarily stopped, and every other sanitary measure that could be carried out was recommended and adopted."

If, now, we analyse these details, we find that, in the Atlantic, the *Apollo* in 1849, the *Renown* in 1865, the *Windsor Castle* and *Lord Warden* in 1866, left places where cholera was prevailing when they sailed, and all of them immediately previous to, or soon after, going to sea, had one or more cases of this disease on board. After being at sea for some time with few or no attacks, these again became more numerous, and after a while ceased. In the *Apollo*

and *Renown* there was a second outbreak; and in the *Jumna*, in 1867, which had not come from a place where the disease existed, her first case was experienced at sea, in nearly the same latitude. The *Windsor Castle*, again, had another case after she was far to the east of the Cape of Good Hope. In the Indian Ocean, the *Gertrude* and *Oriental* in 1859, the *Queen of the North* in 1864, the *Salamanca* in 1865, and the *Durham* in 1866, all left places where cholera existed at the time, and all of them experienced attacks at sea at varying intervals after leaving the land. The *Gertrude* even had two deaths to the south of Mauritius.

We can now examine whether the positions in which these ships experienced their outbreaks agree with those which from other data I have assigned to the cholerific waves proceeding from the southward.

When the *Apollo* left Cork, Ireland was under the epidemic of that year, though the second year of the wave there. The next wave, on June 27th, would be met with in latitude 37½° N., on the meridian of 14½° W.; and on that day this ship, in latitude 35° 21′, had the first case for nine days, and an outbreak commenced which lasted till July 23rd, after which there was a respite for six days. The following wave, on the meridian of 33° W., would be met with in latitude 5° S., on July 27th; and it was after passing this point that the *Apollo* had her second outbreak, commencing on July 29th.

The *Renown* sailed from Gibraltar when the Mediterranean was under the first year of the wave. After the first case, she had no other until she was at 12° N., when the disease commenced again, and continued until she reached 2½° N., when it ceased, and did not recur until three days after, when she got into 4½° S. Having a strong trade at this time, her daily runs were much greater than those of the *Apollo*. The *Renown* would enter the advancing wave on September 15th, when its position on the meridian of 30° W. was about 1¼° S.

The *Windsor Castle* and *Lord Warden* both sailed when England was under the influence of the first year of the wave of the epidemic of 1866. In their progress south, both encountered a fresh wave a little to the north of the Cape de Verd Islands. On August 6th, when the former was in this vicinity, the wave on the meridian of 22° W. would be at 18° N.; and on September 15th, when the other was passing, the wave on the meridian of 20° W., would be at 20½° N. The outbreak in the *Windsor Castle* commenced

about 10° N., within the limit here given for the advancing wave, and that in the *Lord Warden* in 23° N., only a short distance outside its estimated position. In the *Windsor Castle* the last attack was on September 15th, when she was in 37° 20′ S. and 38° 41′ E., the preceding case having occurred twenty-six days before. At this point she was within the influence of another wave, which, on the meridian of 39° E. on that date, would have reached 31° S., and of which distinct indications were soon after experienced all along South Africa.

The case of the *Jumna* differs from those of the other ships already mentioned, in that no cholera existed in England when she left, nor at St. Vincent when she touched there; and the first indication of cholera occurred in her at sea. On July 17th, when the attack commenced, the estimated position of the advancing wave on the meridian of 14½° W. was at 3° N., little more than 200 miles from her place at noon on that day, and choleraic diarrhœa continued to manifest itself until she reached 19½° S., ten days after. It will here be observed that the *Apollo*, *Renown*, and *Jumna*, in years with odd numbers, met these waves in nearly the same latitude, while the *Windsor Castle* and *Lord Warden*, in even years, met with them considerably further to the northward.

In the Indian Ocean, the *Gertrude*, soon after leaving Calcutta, encountered a wave on June 1st at 15° N., near the meridian of 90° E., and the first few cases, up to June 16th, occurred in this. The next case manifested itself on the 20th in 11° S., the estimated position of the approaching wave at that timebeing 13 S. on the meridian of 80° E., or 120 miles only from her actual place. Under this wave, too, she had two cases to the south of Mauritius. The *Oriental* encountered the same wave on June 30th in about 18° N., and in her the outbreak commenced at once.

When the *Queen of the North* left Bombay cholera was pretty active there, and she showed traces of it on her way south. On February 7th she reached the estimated position of an advancing wave in 4° S. and 80° E., and on the following day an outbreak of extreme violence commenced, there having been 37 attacks among 298 persons on board in the next seven days, of whom 24 died. The following year the *Salamanca* left Kurrachee, where cholera was prevailing at the time, and she had some cases immediately after. On May 10th she would meet the same wave, mentioned above in connection with the *Queen of the North*, in 16° N., and the

following day she experienced a considerable increase in the number of cases and of choleraic diarrhœa.

The *Durham* had left the land about ten days before the first case presented itself, and the second occurred ten days later, when on the equator, and five other cases proved fatal in the next twelve days. There were three cases, however, which recovered, and a large number of choleraic diarrhœa during this period, the dates of which occurrences are not available, so that the exact point where the outbreak became intensified cannot be made out satisfactorily. The estimated position of the wave the *Durham* encountered about this time was on March 21st, in latitude 3¾° S. on the meridian of 90° E.

The manifestations of cholera under these different waves were not confined to the ships mentioned in connection with them, but many traces of their influence were found elsewhere as well. The *Gertrude*, though in the same wave as the *Oriental*, had her first case much nearer to the line, and the activity of the causes of the disease on shore corresponded with this, as both in Ceylon and Madras it was much less severe than a few months before, while on the west coast of Hindostan, and still more on the east coast of Africa, it was then very prevalent. As already stated, the *Gertrude* had two cases to the south of Mauritius. There was a considerable outbreak in 1859 at Mauritius; there were also some indications of cholera in the Cape Colony; and a severe epidemic at Zanzibar, commencing in December, clearly showing that the cholerific influences were felt over a very large area, and among communities quite independent of each other.

The series of outbreaks, of which that in the *Queen of the North* to the south of the line in 1864 was the earliest, was a very striking one. The epidemic, as shown above, was followed by that of the *Salamanca* in latitude 14° N. in May the following year. The same year a very severe epidemic ravaged the district on the west coast of Hindostan from 10° to 12° N., which commenced in May. In the same latitude, on the west of the Arabian Gulf, H.M.S. *Penguin* captured some slave-dhows with cholera on board, and one of her people was attacked in April and another in May. The disease also commenced at Aden in May, and subsequently extended into Laheg. At this time, too, it was prevalent nearly as far south as Zanzibar. To the west of Africa, in the middle of the Atlantic, the *Renown* met the same wave on September 16th in 4½° S., and in 1866 the

Windsor Castle and the *Lord Warden* ran into it to the north of the Cape de Verd Islands. In the course of 1866, too, cholera prevailed under its influence in Abyssinia, at Mecca, and along the Euphrates as far as Turkistan; and in 1867 there was a severe outbreak, extending from Constantinople, through Southern Europe, Sicily, and Malta, embracing Tripoli, Tunis, and Algiers in Northern Africa. The influence of this wave was experienced in England and the North of France in 1868 in a manner sufficiently decided to leave no doubt of its presence, though the form of the disease was mostly simple cholera, and the resulting mortality was not so greatly increased as to entitle it to be designated an epidemic.

In like manner, the case in the *Newcastle* and the outbreak in the *Jumna* can be traced from the Cape. In February, 1865, there was a considerable number of severe cases of sporadic or common cholera, and a good deal of choleraic diarrhœa, at Cape-Town. In October, there was a case of ordinary cholera at Port Elizabeth, and another in the *Renown* at sea on November 12th. There were also several cases in coolie ships in quarantine at Mauritius in 1865, and two cases on shore in September. In April, 1866, the *Newcastle* had one case midway between Cape Town and St. Helena, and in the end of 1866, cholera became epidemic at Rio Janeiro. In 1867 H.M.S. *Jumna* had an outbreak in the eastern part of the Atlantic, near the African coast, and in 1868 the further progress of this wave was indicated by the epidemic which commenced at St. Louis on the Senegal in the beginning of December.

The last wave the facts given above embrace is that which was first indicated by the death of the seaman of the *Windsor Castle*, to the south-eastward of the Cape, in September, 1866. In October of that year, a lady, who had not been out of the colony, died in Cape Town of cholera, and in December, choleraic diarrhœa suddenly showed itself all over Maritzburg, in Natal; and in February, 1867, choleraic diarrhœa became very common at Cape Town and Port Elizabeth. In January, 1868, cholera appeared at Buenos Ayres and Monte Video as an epidemic, showing, as in the instances previously given, that the cholerific influences were very widely spread, and were progressive, and that the aggravation of the disease in ships was merely an indication of a cause or series of causes of general operation.

ON THE PREVAILING EPIDEMICS OF THE LAST THREE YEARS.

AN ADDRESS TO THE EPIDEMIOLOGICAL SOCIETY, NOV. 9TH, 1871, ON TAKING THE CHAIR.

By ROBERT LAWSON, Esq.,

INSPECTOR-GENERAL OF HOSPITALS, ROYAL ARMY.

[*Read November 9th, 1871, on taking the Chair.*]

It has been usual for the occupants of this chair to address you at the first meeting of the Session on some point connected with Epidemiology. The subject of their addresses, and its manner of treatment, must naturally vary according to the matters requiring attention at the time, and the views of the individual concerning them, but there will always be a certain latitude for selection, and an opportunity for laying before you certain opinions, which may represent more or less closely those laws which nature follows in the development of epidemics, which it is our special object to investigate.

We can ascertain the laws which nature follows in the generation and diffusion of epidemics only by tracing out the different factors she employs, and there is no method of effecting this but by embracing in our investigations every circumstance connected with the manifestation of the disease, not only in particular localities, or among certain individuals, but for lengthened periods and over large spaces, that every phase of her operations may be included. Our present acquaintance with the natural history of disease (using that expression in its most extended sense) is too defective to afford the basis here contemplated for a satisfactory induction, and it is essential for the progress of our knowledge it should be increased.

In one branch of the subject epidemiologists have long felt the want of fuller accounts of the epidemics of former

days, and the importance of recording the progress of those which are passing around them, while trustworthy evidence concerning them is available. In furtherance of this object, the late Dr. McWilliam, who for many years was secretary to this society, on several occasions drew up a short sketch of the epidemics which appeared in different parts of the world during the previous year, which was given in the President's address for the time, and Dr. Milroy, one of our members, now in the West Indies investigating a recently proposed method of treating leprosy, has particularly dwelt on the importance of continuing this practice, and has from time to time made valuable contributions on the subject, both to this society and to the British and Foreign Quarterly Review, from which I have derived great assistance in prosecuting some of those investigations, the results of which have been placed before you.

Such a sketch as is here alluded to, though useful, must necessarily be imperfect, as there are many parts of the world from which little information has hitherto been obtained, and that derived from others is usually conveyed through the medium of the newspapers, which seldom do more than notice the prevalence of a particular affection, without that fulness of detail which is desirable; and much disease, specially interesting in an epidemiological point of view, may pass unnoticed even in our own country, and of course much more in others where the machinery for registration which we possess does not exist. It is important, however, to collect such notices as they appear, as however incomplete in themselves they will ultimately constitute a very useful body of facts.

The registration of deaths which has been introduced into the British possessions in India, though still far from perfect, is becoming of more value every year, and will soon afford a most important aid to the epidemiologist; and I am glad to say there are indications that the epidemics of Persia, of which hitherto we have known so little, are likely to be recorded. Dr. Tholozan, physician to the Shah, and a corresponding member of this society, has, with the authority of the government, appointed a staff of qualified observers, who are distributed in the chief places in the country, extending from Bushire and Shiraz in the south to Tabriz in the north, and from Meshed in the east to Hamadan and Kermanshaw in the west. These observers report to him at Teheran on the state of health of the population in their vicinity, and, through their instrumentality, he has been enabled to

compile an account of cholera in Persia from 1820 to the present time as well, which will fill up an important blank in the history of this disease. The agents of the Board of Health of Constantinople now report the occurrence of epidemics to that body, and we have received from Dr. Dickson, our corresponding member there, notices of them on numerous occasions. It would be very satisfactory to obtain information from the Russian provinces in Asia as well as in Europe on these points, and I am in hopes Mr. Michell, one of our members, will be able to present us with a paper drawn from the official reports of the government on the recent epidemic of cholera. Dr. J. W. Moore, of Dublin, has undertaken to bring to the notice of the society any facts he may obtain from the medical publications of Denmark, Sweden, or Norway, bearing on cholera and fever and other epidemics. Of course, information from other parts of the world is desirable, but that from the countries named possesses a special interest for us at present, in connection with the migration of cholera from India to Europe generally, and to this country in particular.

Of the epidemics which have prevailed during 1870-71, the first I will allude to is small-pox; the deaths from this disease had declined pretty regularly in England from 373 per million living in 1864, to 72 only 1869. In the third quarter of 1870 they showed a disposition to increase, and, in the fourth they were about thrice as numerous as during either of the first two, there being 1229 in all. In the first quarter of 1871 they increased to 4903, and in the second to 7012. The number of attacks seemed to culminate about the beginning of June, 1871, subsequent to which their frequency diminished, rather quickly, until about a month ago, when they became rather more numerous again at various points. The force of the epidemic was nearly confined to certain districts of the country, viz., London and its vicinity, Liverpool and its vicinity, including Manchester, Salford, Bolton and Wigan, the coal districts of the north, and the mining districts of South Wales; but several other places had pretty severe attacks, such as Southampton, Weymouth, Falmouth, Stoke-on-Trent, and Great Grimsby.

Coincident with this epidemic in England and Wales there was a very severe one in Holland. When this commenced I have no information, but the returns for the four principal towns show a considerable mortality in December 1870, which was considerably increased in January, 1871; the deaths attained their maximum at The Hague in January,

at Utrecht in February, at Rotterdam in March, and at Amsterdam in June, after which in each case they decreased, and in July, with the exception of Amsterdam, had become comparatively few. This simultaneous increase and decrease of the epidemic in Holland and England is a circumstance worthy of notice, and indicates the operation of some cause common to both. There is a tendency in epidemics of small-pox, in London at least, to diminish about the end of the second quarter of the year, and in several this has been well marked. In others, however, the opposite has taken place, and the mortality during the third quarter has considerably exceeded that during the second. The mean result decided from the returns for the 31 years, 1840 to 1870 inclusive, is that 27 per cent. of the annual mortality from small-pox takes place in the first quarter of the year, 25 in the second, 22 in the third, and 26 in the fourth.

In Scotland small pox was at its minimum in 1868, there having been 15 deaths only in the whole country; in 1869, and still more in 1870, it showed greater activity. In the former year there were 37 deaths in Greenock from the disease out of a total of 51 in the eight principal towns, and in the latter 36 in Greenock and 26 in Glasgow, out of a total of 74 in the eight towns. In two of the northern and in the southern provinces of Sweden small-pox was increasing in 1869. Vaccination seems to have been more carefully enforced in Scotland than in England, and hence the smaller mortality, but the greater number of deaths show clearly an increased activity in the causes of the disease in 1869 and 1870, which, as in England, was strongly developed in a comparatively limited locality only.

In 1868 there was a severe epidemic of small-pox in the Madras Presidency, which caused 34,330 deaths; in 1869 the deaths there from this disease fell to 18,000. In the Bombay Presidency, in the end of 1868 and first half of 1869, small-pox was very prevalent, and in the latter year it was very fatal in Berar, the Central Provinces, Oude, the North-west Provinces, and in the Punjaub; in the last named province the deaths from small-pox alone amounted to 53,195, nearly six times the mortality from the cholera epidemic of the same year of that district.

Small-pox seems to have been very widely diffused in 1870 and 1871. In the former it was prevailing in the early months at Marseilles, Lyons, Dijon, and Bourdeaux, and later at Paris. It was prevailing in Malta in November and December, at Teneriffe from August; it was also prevailing

in Ceylon, Hong Kong, and Japan in December, and at the last especially as a severe epidemic. In the western hemisphere it was frequent at Buenos Ayres from the end of 1869, and at Cayenne in September. In 1871 the disease was very severe in Teneriffe in the early months. It was at Gibraltar from May, and continued in Malta, and was also pretty frequent at Florence, Vienna, and Brussels, and parts of France. In the early months it was common at Bushire in the Persian Gulf, at Bombay, Allahabad, Madras, and the Straits Settlements, and continued in Ceylon, Hong Kong and Japan. In the western hemisphere the disease continued at Buenos Ayres early in the year, and was at Berbice in March and April, while there were a few cases in Barbadoes and the Bahamas, in New York and Canada, and there seems to have been a pretty severe outbreak at Cape Coast, in Western Africa, in July and August. Imperfect as these details no doubt are, they show an unusual activity of the causes of this disease over a very large portion of the world.

The epidemic in this country has been more severe than any which has occurred since 1847. Those of 1848 and 1852 produced a mortality of 4 in 10,000 only, and that of 1864 just under 4 in 10,000, while in the first half of 1871 the mortality from small-pox in England and Wales was just over 5 in 10,000, and when the other half year is included will be considerably more. But severe as the epidemic was in England it was much more serious in other countries; the deaths caused by it in the four large towns in Holland in the eight months from December 1870, to July 1871, inclusive, amounted to 5636 in a population of 558,570, or 101 in 10,000, while in London during the nine months from October, 1870, to June, 1871, the deaths were 6225, and in Liverpool 1905, being in the ratio of 19 and 39 to 10,000 respectively. Had the ratio in the Dutch towns been met with in London the deaths would have amounted to nearly 33,000, and in Liverpool to 5000. Much of our immunity is no doubt owing to the more general diffusion of vaccination in this country, but even here there is much room for improvement in this respect.

Measles show a diminishing ratio of mortality in England and Wales from 1868, when the deaths were 5.4 in 10,000, to 1870, when they were a little over 3 in 10,000 only, and in the first half of 1871 they remained much as in the corresponding period of the previous year. They were prevalent in Portsmouth and Bristol during the first quarter of 1870; in the second in London, Bristol, and Bradford; in the

third at Bradford and Leeds; and in the fourth at Norwich, Wolverhampton, Liverpool and Leeds. In the first quarter of 1871 Birmingham, Liverpool and Manchester were the places chiefly affected, and in the second these places, with Salford, and their vicinity, had a sharp epidemic. In Scotland, in 1870, measles were very prevalent in Paisley and Greenock, but to a moderate extent only in the other large towns. Of this disease in other countries I have no information for 1870, and for 1871 very little; it seems to have been frequent at Cape Town during the first half of the year, and is mentioned as being at the Straits Settlements in April, and at Allahabad in June.

The progress of scarlatina in England and Wales for the last few years has been very different from that of measles. In 1867 the deaths from scarlet fever were 6 in 10,000, in 1868 10, in 1869 12.7, and in 1870 they reached 14.4. The intensity of the epidemic seems now abating, as not only were the deaths during the first and second quarters of 1871, together, less than those in the fourth of 1870, but they were 2370 less numerous than during the corresponding quarters of that year.

In the first quarter of 1870, of the 17 large towns which the Registrar-General specifies in his reports, scarlet fever was unusually prevalent in London, Portsmouth, Bristol, Nottingham, Liverpool, Bradford, Leeds, and particularly so in Sheffield, and Hull, and the disease was experienced in their vicinity, and also in the mining districts in South Wales. In the second quarter the disease was still frequent in all these towns, though less active, except at Bristol, where the deaths were more than doubled; the mining districts in South Wales, those in Durham, and some points in Cornwall were also considerably affected. In the third quarter the activity of the disease increased materially in London, Norwich, Bristol, Birmingham, Leicester, Nottingham, Liverpool, and Bradford. At Hull it was little different, but was less at Sheffield. South Wales, and some points in Cornwall and Devonshire continued to suffer; also Darlington, Stockton, and Auckland in Durham. With reference to Bristol, the Registrar-General states, "the 293 deaths from scarlet fever, the 52 deaths from fever, and 156 from diarrhœa in three months, imply some essential defects in dwellings, sewage, water supply, or other hygienic conditions, which mere quarantine arrangements do not meet." In the fourth quarter the epidemic was greatly intensified in many places, and was experienced in every county with variable force; of the large towns it was most active in

Norwich, Bristol, Leicester and Liverpool. In the first quarter of 1871 there was a marked diminution of deaths from scarlet fever in all the large towns except Sunderland, in which the rate of the previous quarter was somewhat increased; the same decrease is met with in the various counties, and this was continued into the following quarter, with the exception of Sheffield and Sunderland, in both of which there was some further increase of mortality from this disease.

In the eight principal towns of Scotland the deaths from scarlatina in 1868 were 1,797, in 1869 2,250, and in 1870 1,205 only, being in the ratios of 19, 23, and 12, to 10,000, respectively. The epidemic of 1868-9 thus passed off in 1870 to a great extent, instead of increasing in the third quarter of that year as it did in England. Of the progress of scarlet fever in Scotland in the present year I have, unfortunately, no information. There appears to have been a few cases of this disease in Canada in 1871, but I regret I have met with very few notices of it in other countries for several years past. Of these the following are interesting in connection with what has been given above. At Berlin the deaths from scarlatina in 1868 were 600, in 1869 they decreased to 187, and in 1870 still further to 95, shewing the epidemic of 1868 to have been gradually passing off. In Sweden there was a very decided increase in the frequency of and mortality from scarlatina in the second half of 1869, corresponding in this respect with England and Scotland. In Philadelphia in the United States the mortality from this disease in 1869 was 10 in 10,000, far higher than any year since 1861. It is clear, however, that much more numerous and detailed notices of its prevalence in other countries are required before its character as an epidemic can be fairly made out.

Diphtheria has presented but slight fluctuations in England and Wales since 1866, and has caused a mortality varying from 1·2 to 1·4 in 10,000 per annum. In 1870 the mortality was only 1·2 in 10,000 and the deaths in the first two quarters of 1871 were but slightly more numerous than in the corresponding periods in 1870. During the first quarter of 1870 the disease was above the mean in Portsmouth, Bristol, Birmingham, Manchester, Leeds, and Sunderland; and in Wolverhampton the mortality was about five times greater, or in the annual ratio of 6·1 per 10,000. In the second quarter the annual ratio of deaths in Wolverhampton was still 5·5; while in Norwich, Bristol, and Brad-

ford, the deaths were above the mean. In the third quarter there was no death in Wolverhampton from diphtheria; Birmingham, Leicester, Bradford, and Hull, were above the mean rate, and Norwich, and Sunderland, had the annual ratios of 3, and 2·8 per 10,000 respectively. In the fourth quarter Leicester and Liverpool were a little over the mean; Norwich, Birmingham, and Bradford had about twice as many, and Sunderland reached the high rate of 6·3 per 10,000 per annum. In the first quarter of 1871 Birmingham, Bradford, Leeds, and Sunderland, still showed high rates of mortality from the disease; and, in the second, Birmingham, Liverpool, and Newcastle-on-Tyne. Though it seems to have attained its chief development in the places named above, diphtheria was extensively diffused through the country during the year and a half under review, so much so that one or more deaths were caused by it in every county in England and Wales in each of the six quarters, except Cumberland, Westmorland, Huntingdon, Rutland, and Buckingham, which were each without a death for one quarter, and Bedfordshire, which was exempt for three.

In Scotland, in 1869, the mean mortality of the eight large towns from diphtheria was 1·9 per 10,000, and this rate was exceeded in Glasgow, Edinburgh, and Leith. In 1870 the rate for the towns generally rose to 2·7 per 10,000; Glasgow, Greenock, Leith, and Edinburgh were those in which the ratio exceeded the mean of 1869, and in Edinburgh it was no less than 5·2 per 10,000 on the whole year. Scarlatina was prevalent in these towns in 1870, and particularly so in Leith, where the deaths from it amounted to 22 in 10,000; yet in Dundee, when they rose to 25 in 10,000, those from diphtheria were ·8 only in the same number. Great as was the mortality from diphtheria in the towns, Dr. Stark remarks that, in Scotland, it has always been proportionally much more fatal in the rural districts.

On the Continent there seems to have been a very severe epidemic of diphtheria in the Roumanian towns along the Danube in 1869; the same year in Berlin it carried off 873 persons, and in 1870, 555;* of its course in 1871 I have no information. In November and December, 1870, diphtheria was not infrequent at Malta, and it continued up to August of the present year. At Mauritius, too, this disease was met with from March, 1871, onwards.

* In Sweden cases were met with in all the provinces in 1869, but their number was worthy of note in limited tracts of the eastern, southern, and western coast districts, only.

Hooping-cough has not prevailed to an unusual extent for the last three years in England and Wales. The rate of mortality in 1868 was 4.3 in 10,000, in 1869, 5, and in 1870 just under 5 in 10,000. In the first two quarters of 1871 the deaths in the Registrar-General's returns amount to 4,805, against 7,464 in the corresponding period in 1870, or about two-thirds the number; so that the disposition to the disease seems on the decrease. Like diphtheria, hooping-cough was generally diffused over the country; yet, like that, too, it was intensified in different localities from time to time. In the first quarter of 1870 it was prevalent in London, Bristol, Birmingham, Nottingham, Leeds, Newcastle-on-Tyne, and particularly so in Manchester and Salford. In the second quarter, it was much less active everywhere. In the third quarter, Liverpool was the only one of the large towns which showed any considerable amount of it; and, in the fourth quarter, Norwich, Leicester, and Liverpool, and to a minor degree Sunderland. In the first quarter of 1871, Norwich, Birmingham, Liverpool, and Sunderland, all presented a considerable increase of mortality from the disease, and London and Sheffield to a lesser extent; in the second quarter, Birmingham, Liverpool, Sunderland, and Newcastle were still under its influence in a marked manner.

In Scotland, in 1869, the mean death-rate from hooping-cough in the eight large towns was 14 in 10,000, and in 1870 10·4 in 10,000 only, showing a decrease in the latter year considerably greater than in England. In 1869, the mean rate was exceeded in Glasgow, Paisley, Edinburgh, and Leith, while Greenock was below it, and Aberdeen, Dundee, and Perth considerably so. In 1870, Glasgow, Greenock, and Dundee exceeded the mean; Leith coincided with it; and Edinburgh, Paisley, Aberdeen, and Perth all fell much short of it. These remarkable fluctuations of disease in places so close and in such constant communication with each other as Edinburgh and Leith, or Glasgow, Paisley, and Greenock, indicate the operation of some factors of local influence which have not yet been appreciated.

There has been an unusual prevalence of fever, not only in Europe, but in other parts of the world for some years past. In Europe the disease has assumed, at different points and times, the various forms in which it appears as an epidemic—viz., the yellow, relapsing, typhus, and remittent, and there is such a connection between these, in point of time and locality, that one cannot be separated from the

other without breaking up the relation they have to each other as constituent parts of the same general epidemic. To illustrate this point, I will endeavour to sketch the progress of fever in Europe and its immediate vicinity from 1868, so far as the rather scanty notices in my possession will allow. In the early part of that year, fever of a very virulent character, described as typhus, became epidemic in Tunis and Algiers, on the south coast of the Mediterranean, and soon thereafter this disease, it was said, in various forms, became extensively diffused in Spain. In the city of Naples, typhus caused 1286 deaths up to the end of August; and in the western part, at least, of Asia Minor a malignant fever raged through the country during the whole summer. In Berlin, relapsing fever became frequent this year, and it was prevalent in Silesia; some cases also appeared in London in the course of the summer, and towards the end of the year at Tredegar in Monmouthshire. In Scotland, where deaths had been recorded from this disease every year since 1865, the returns of the Registrar-General show 45 from it in 1868, which were not confined to one point, but distributed over 15 counties out of the 33 into which the country is subdivided: these are associated with a large number of typhus and enteric cases. In Sweden, relapsing fever attracted attention in the end of 1867, and in 1868 the cases seem to have been more frequent, accompanying typhus, and what are called gastric fevers; so that this peculiar form, which has attracted considerable attention lately, was not confined to one or a few points, but was met with over nearly half of Europe.

In 1869, fever prevailed extensively through Europe. It was rife in Spain, I believe, during most of the year. It was also very prevalent in Naples, and early in the year at Paris, Brussels, Breslau, and there was a severe epidemic of the relapsing fever at Berlin. In Russia fever was epidemic in the Governments of Toula, Pskov, Novgorod, and Olonetz, from February onwards; but the period of cessation I have not seen mentioned. In England, the general death-rate from fever in 1869 was 8·5 in 10.000, against 9·2 the previous year; but in several places it continued to be largely developed, and caused many deaths. This was the case in Islington, some parts of Monmouthshire, Lancashire, and Durham. Relapsing fever also continued at Tredegar from the commencement of the year, accompanied by typhus; the former showed itself in London again in May, and in Liverpool in September; but in neither was the disease much

developed this year. In Scotland deaths from relapsing fever had been registered in 1868 in Glasgow, Paisley, and Greenock; in 1869, they were met with in increased numbers, and in Glasgow, Paisley, Edinburgh, and Dundee. In Sweden fevers continued to prevail, if not with quite the same severity, yet extending over portions of the country which had previously been comparatively free from them, and ague became common over the whole country, except Jemtland, but more particularly round the Malo Lake, north of the Wener Lake, and in Ostergothland and Kalmar, the coast districts south of Stockholm.

In 1870, fever in the yellow form appeared on the south coast of Spain, and in the course of the summer Barcelona, Valencia, Alicante, and Palma in Majorca, were affected. This form of the disease was occurring at the same time at New Orleans, Mobile, and even as far north as New York, where numerous cases presented themselves among a body of troops quartered on Governor's Island in the harbour. In Berlin the deaths from fevers of all descriptions which in 1869 were 567, rose in 1870 to 701, showing a considerable increase. In England and Wales the general death-rate from fever in 1870 was 8 in 10,000, a slight increase over that of 1869, though under the average of the previous twenty years, which was 9.18. Taking the mortality by divisions, it was only in Yorkshire, and the northern division, that the mortality from fever was in excess of the average for the ten years, 1851-60, and in both instances the rates of 1870 were about one-fourth above the average. The most striking feature in progress of fever in this country in 1870 was the great development of the relapsing fever in London, in Liverpool, and in a less degree at Merthyr Tydfil, and at Leeds. In Scotland, in the eight large towns, the mortality from fever was reduced from 19.7 in 10,000 in 1869 to 14.5 in 1870; but the portion of this attributed to the relapsing form rose from 0.0 per 10,000 in the former to 1.5 in the latter; and while Greenock, Leith. Perth, and Aberdeen had no death from the disease in 1869, Perth alone of the eight showed this immunity in 1870, while in Glasgow the deaths attributed to it were 2.5 in 10,000, and in Paisley 2.8, showing a great frequency of the disease.

In 1871 the notices of fever in Europe in my possession are very few. There seems to have been a severe epidemic of typhus in Vienna, which, during the twenty-one weeks from 22nd March to 15th August, led to the admission of 1766 cases to hospital, of which 366, or 22 per cent., died.

How far this epidemic extended in the Austrian dominions, or involved other countries, unfortunately I have no information. Fever in this country was rather less active in the first two quarters of 1871 than in the corresponding periods in 1870.

In Mauritius, since the severe epidemic of 1867, fever has been more prevalent than for some time previously, and has every now and then been severe. Early in 1869 there was a severe epidemic of the same character in Bourbon. In 1870, after the cessation of the cholera epidemic at Zanzibar, dengue appeared there, and seems to have become general. In 1871 this disease broke out at Aden towards the end of June, subsiding apparently about the middle of September; during this time 730, out of a total strength of 972 men women and children of the garrison of Aden, were attacked. The disease was said to have been prevailing extensively at Zanzibar, and probably along the coast to the northward. Fever, said to resemble that at Mauritius, had made its appearance in Madagascar in the early part of 1871, coinciding with an increase of the disease in Mauritius, and at Cape Town fever was very prevalent in the early months of the year.

In India, in 1869, fever was very wide spread, with both cholera and small-pox, though the mortality from fever exceeded that from both the others. According to data given by Dr. Cunningham, Sanitary Commissioner with the Government of India, the deaths from fever appear that year to have amounted to 78 in 10,000 of the population in Oudh, to 83 in the North-western provinces, to 142 in the Central provinces, and to 156 in the Punjaub. The details given by Dr. Cunningham do not permit of the geographical distribution of fever and cholera being shown with accuracy.

Early in this year (1871) a fever resembling plague was reported as having broken out in Khorasan, in Persia; and at Tehran, typhus was subsequently mentioned together with cholera. As in India, however, the details are not sufficient to show the relation of these diseases.

Fever prevailed extensively in Peru in 1868 and 1869, being in the yellow form along the coast at least; it also was prevalent along the Pacific coast of Nicaragua, and in Venezuela and Caraccas, in the latter year. On 21st July, 1869, the existence of yellow fever at Rio Janeiro was officially notified; and in the early months of 1870, the summer of that country, the disease was severe, though principally affecting foreigners. About this time, some few

persons who had arrived at Buenos Ayres from Brazil were attacked with yellow fever; but the disease soon ceased. In 1871, however, many months after the termination of the cases just mentioned, a severe epidemic commenced at Buenos Ayres at the end of January; it culminated in the week ending 14th April, and by the end of May seems to have ceased. The number of deaths has been variously stated; the official account gives 13,402 up to the 12th May. Another, in the Buenos Ayres *Standard*, estimates 26,000 as having died to the end of April. The former may have underrated the mortality; but as the latter deals with no number under 100, it looks, to say the least of it, extremely suspicious. Assuncion, on the Paraguay, about 150 miles above its confluence with the Parana, like Buenos Ayres, had a slight attack of yellow fever in the summer of 1869-70; and again a severe epidemic at the end of 1870. Corrientes, at the confluence of the rivers, suffered severely from the same disease early in 1871. At this time, too, the existence of yellow fever was reported in some of the ports of Chili, in the latitude of those just alluded to, but on the west coast of South America; but the evidence is not quite satisfactory. During 1871 yellow fever was prevalent at Bahia, Maranham, and Para; and at Pernambuco a fever which, from the account given of it, resembles cerebrospinal fever in some respects, had been frequent for some months previous to August.

These details afford several illustrations of the law I have indicated as that pursued by epidemics of fever. Thus the severe outbreak in Asia Minor and along the shores of the western part of the Mediterranean occurred in a year with an even number, and in the following, an odd year, the fever extended to the northern parts of Europe. The outbreak of yellow fever along the coast of Spain in 1870, accompanied by a similar manifestation at New Orleans and Mobile, is to the same effect. The very severe outbreak of fever in Central and Upper India in 1869, and that in Khorassan in 1871, both occur in a zone in which the law shows the weight of febrile epidemics is to be expected in years with an odd number. The occurrence of an epidemic at Rio Janeiro in the end of the odd year, and at Buenos Ayres at the commencement of the odd year, are all in accordance with the general law.

Cholera in its epidemic form was last met with in the western part of Europe in 1867, when it was confined to countries bordering the Mediterranean. Though there were

distinct indications of a cholerific influence passing over this country both in 1868, and in 1870, the disease as an epidemic has not threatened us until now; to trace this it is necessary to go back for some years. It will be recollected that, in 1867, a severe outbreak of cholera took place in Northern India, and the same year it extended from that westward as far as the southern shore of the Caspian, affecting a portion of Persia in which the epidemic disease had not been seen for some years, though it had existed through Mesopotamia and to the west of the Caspian in 1866. During the winter of 1867, and early months of 1868, the disease continued pretty active on the south and east shores of the Caspian, but seems to have subsided towards the middle of the year. In August and September, however, the disease resumed its activity and seems to have been felt severely over the districts in which it appeared in 1867, but, up to September at least, did not extend westward over those in which it had prevailed in 1866. Early in 1869 a serious outbreak of cholera occurred at Herat and Ferah, and a little later the severe epidemic in Central and Northern India took place, showing great activity of the causes of the disease in India and parts of Persia at that time. While that was going on, cholera made its appearance at Kiew, in southern Persia; the first case showed itself in the middle of May and proved fatal, the next was a month later, and in July and August they became more numerous, and the disease reached its climax in October. There was no trace of importation in this instance, and there is no account of cholera being in existence nearer than the south of the Caspian. In October the disease extended to different parts of East Orel to Jitomir, and descended the Dneiper to Tcherkassy; in November it had reached Retchiska, also on that river, but to the north of Kiew; it was also at Piriatin, in the government of Poltava; and in December at Roslave, in that of Smolensk; it became gradually diffused in short through the valley of the Dneiper, and, though nowhere this year did it appear as a severe epidemic, yet it was unequivocally malignant cholera. In July, 1870, the disease had become active again, and was experienced along the north-east coast of the Black Sea from Soukhem Kaleh to Kertch, and embracing various points as far as Tsaritzin on the Volga, and extending to the north-westward as far as St. Petersburg, which it reached on 17th August, and where, between this date and the 28th October, it caused 536 deaths. During the colder months it was much

less active, but there was a continued succession of cases, and in 1871, as the summer months came on, these became more numerous again, and the disease spread into the provinces bordering the Baltic, to Helsingfors in Finland, and even as far as Archangel, also into Siberia as far as Tomsh. In July the disease began to move westwards, and was at Wilna early in the month, at Konigsberg towards the end of it, at Swinemunde and Hamburg on 23rd August, while cases had occurred at Altona as early as the 19th, it had also declared itself at Elbing, Dantzic, and Berlin, during the course of that month.

Several now present may recollect that under the general law of the progress of cholera epidemics the chief influence of these is experienced in Persia in a year with an even number, in southern Russia in the following or odd year, and in northern Russia in the next or even one. The outbreak in Persia in July and August, 1868, that at Kiew in 1869, and at St. Petersburg in 1870, are quite in accordance with this law. Another very important feature in the history of cholera is illustrated by the details given above with regard to fever, viz., its inability to penetrate a district as an epidemic which is occupied by fever. Though we have no precise information of the distance eastward to which the fever of 1868, 1869, and 1870 extended, yet there are the great facts that in 1868, while cholera was severe in Persia, fever was no less so in Asia Minor and through the Mediterranean; in 1869, when the former advanced into Russia, fever extended from Spain to Silesia, and was particularly severe in Russia itself, in the governments of Pskov, Novgorod, Olonetz, and St. Petersburg in their centre, though how far it involved other portions of that country unfortunately there is no information. The continuance of this bar to the western progress of cholera was indicated by the increase of fever at Berlin in 1870, and the epidemic at Vienna early in 1871, and, I have no doubt, that it will ultimately be found that in those districts along the south shore of the Baltic, through which it ultimately made its way, its progress was preceded by a diminution of any fever there existing. This remark applies to all descriptions of fever except enteric, which is found to increase at the same time and place with cholera. A third point most important to be kept in mind is the fact of the disease having commenced with sporadic cases, 300 miles from the nearest coast, and without any trace of importation, in fact we have no information of the existence of cholera at the

time nearer than the south of the Caspian, some 1500 miles off. Many maintain that sporadic cases, though they may present every characteristic symptom of those occurring in the middle of an epidemic, are essentially different in their nature, apparently because they frequently arise without communication with previous cases; yet here is an epidemic which has so arisen, and if an epidemic can spring up without such communication, it is clear sporadic cases may do so, though the causes are not in sufficient force to generate an epidemic. On the other hand, the appearance of sporadic cases in the malignant form affords unequivocal evidence of the extension of the cholerific influences to the spot where they occur, whether preceded by importation or not.

According to the law already alluded to, the wave of cholera which overspread Persia in 1868 and southern Russia in 1869 would retire from the former in 1870, and the latter in 1871, and might or might not be succeeded by another, the reasons for which we are not yet acquainted with. As it happens another has followed. This was indicated by outbreaks at Kerman, in the south-east of Persia, and at Buroghird and Hamadan in the west, early in the summer, and somewhat later at Bagdad. In 1871 the disease extended westward in Persia, affecting Bushire in February, Bassora in March, and attacking Nejef, Kerbela, on the Euphrates, and down the west coast of the Persian Gulf. It was also at Teheran severely at the beginning of June, and at Turkomantchai and Tabriz apparently towards the end of that month. While these outbreaks were showing themselves in Persia several of the Russian ports in the Black sea which had been free of the disease for some time were again attacked; among these were Taganrog, Rostov, and Kherson, and it spread into the provinces of Poltava and Kostroma in a very active form, and reappeared in Nicolaief, Ekaterinoslav, and Kiew, thus corresponding in its advance to the preceding wave. The disease still manifests a disposition to advance westwards, both in Arabia and at Constantinople, Broussa, and Smyrna. Three cases presented themselves in Vienna in July. This epidemic is considerably more intense than that which preceded it in Russia, and as the conditions which so long confined the latter to Russia seem disappearing, and the disease has crept along the south shore of the Baltic as it did in 1831, it seems very probable that we may experience an outbreak in this country next year corresponding to that of 1832.

In the end of 1869 there was a severe outbreak of cholera at Zanzibar, extending into 1870; this embraced the coast to the south as far as Cape Delgado, and for some distance north of Zanzibar, and also extended inland. The islands Comoro and Johanna were attacked, but the other three of the group, as well as the Mozambique settlements, escaped for the time, it was supposed in consequence of a strict quarantine. In August, however, Mozambique also was attacked, and in September the disease showed itself on the north-east coast of Madagascar. In the early months of 1871 cholera was prevalent on the east coast of Madagascar, also it is said in the islands of Johanna, and Mayotta another of the Comoro group, and it had also appeared at the mouths of the Zambezi, to the south of Mozambique. Sporadic cases were met with at this time in Mauritius. The last mentioned outbreak corresponds both as to time and locality with a fresh wave from the southward. The cholerific influences were evidently experienced at Mauritius, but owing to the frequency of fever there, most probably, were held in check. The prevalence of dengue at Zanzibar after the cholera of 1870, and its extension to the northward, of which we have heard from Aden, have most likely limited its extension to the north. Whether the dengue or other fevers succeeded the decline of cholera at Mozambique last year I have no evidence, but so far, the latter does not seem to have returned there. Along the east coast of Madagascar the cholera of the early months of 1871 seemed to be succeeded at a later period by a fever resembling that of Mauritius.

This hasty sketch embraces the chief epidemics of the last two years, as far as the information in my possession enabled me to refer to them. I had originally intended to notice some of the epizootics which have appeared within the same time, but the length this communication has reached renders it advisable to finish for the present.

THE INFLUENCE OF VACCINATION,

AGE, SEX, AND OCCUPATION ON THE MORTALITY IN SMALL POX.

By ROBERT GRIEVE, M.D.,

MEDICAL SUPERINTENDENT OF THE HAMPSTEAD HOSPITAL.

[*Read April 10th*, 1872.]

In March, 1871, I brought under the notice of this Society an analysis of 800 cases of small-pox treated in the Hampstead Hospital during the existing epidemic. In the present paper I purpose giving, in a more detailed form, the statistics of a larger number, and from these figures endeavour to show the influence exercised by vaccination, age, sex, and occupation on the rate of mortality in small-pox. I confine myself to personal experience, trusting that a contribution towards the history of the present fatal outbreak by one who has had very great opportunities of observing it, may possess interest.

Vaccination.—To show that the mortality amongst patients suffering from small-pox, and who have been previously vaccinated, is very much less than it is amongst those who are not so protected, is, at the present day, but repeating a truism; still, additional figures can do no harm, so I give them. Of 6,221 patients admitted, 1,248 were without marks of vaccination, and of these 638, or 51·12 per cent., died; whilst among the 4,973 who showed proofs of being vaccinated, in only 567 instances did the disease prove fatal, giving a percentage of mortality of 11·40. From these numbers it is seen, that although the number of patients received into the Hospital of the vaccinated class exceeded the number in the unvaccinated, a fact of which the antivaccination League has made vigorous use, the larger number of deaths occurred amongst the unvaccinated.

The general percentage of mortality is 19·36, which is above the average of late epidemics. This has been ascribed by Mr. Marson, who has noticed the same circumstance at the Small-pox Hospital, and whose lengthened experience entitles him to speak on this point with authority, not only to the form of the disease generally being more severe, but also to the large proportion of cases of the malignant and hæmorrhagic type which have come under treatment. This great prevalence of hæmorrhagic small-pox is characteristic of the present outbreak, and seems to depend upon a peculiar development of the epidemic influence.

It is possible for individuals to be vaccinated, and yet not to obtain all the protection they might do, as the vaccination may be inefficiently performed. It may be inefficient either in quality or quantity. As to quality, where there are different observers, with possibly different standards, it is very difficult to reduce the results to figures: so, while believing firmly that the character of the vaccine pock exercises an immense influence over its protective power, I am unable to bring statistics to confirm my belief.

With quantity we have not the same difficulty; there are the marks, and they have but to be looked for and counted.

In the 3,555 cases in which the number of marks was noted, and in which the results are given in full in Table I, it is found that the percentage of mortality in those showing one mark is 17·39, showing two 12·17, showing three 10.58, showing four 8·38, showing five and more 6·43—a scale in which the mortality is in inverse ratio to the number of marks seen. The practical deduction to be made from these numbers is that the larger the number of places in which we vaccinate the more protection is given. To obtain even a fair average of protection, at least three marks are required, but something is gained by exceeding that number.

An interesting study in connection with the influence of cow-pox over small-pox is to watch the diseases when concurrent. A considerable number of children have come as patients into the Hospital suffering from both vaccinia and variola, and my experience is, that if the cow-pock reach the eighth day before the small-pox eruption makes its appearance, the latter disease is modified. As the result of the investigations I have been able to make into the question of the period of incubation of small-pox, I have come to the conclusion that in the great majority of cases the small-pox eruption shows itself on the fourteenth day after the recep-

tion into the system of the variolous influence; vaccination during the first three or four days of the period of incubation will accordingly be useful—a strong reason for vaccinating persons in a house where small-pox has already shown itself.

Of small-pox after re-vaccination I have not seen much, owing, I believe, to the rarity of its occurrence. Out of the 6,221 cases above mentioned, in only three could any satisfactory proof of previous re-vaccination be discovered. A good many of the patients said, on their admission, that they had been re-vaccinated; but, on pressing the inquiries, it was found that while the operation had been performed no after effects had been produced, and that thereupon the doctor had assured them, that as they were not susceptible to the vaccination there was no fear of their taking small-pox. Their presence in the hospital was sufficient proof of the fallibility of this doctrine, one which is inculcated even yet by many members of the profession.

Our nurses and servants in constant and close attendance on small-pox, when protected by revaccination, do not take the disease; and in this respect the experience at the Hampstead Hospital coincides with that at the older Institution at Highgate. I wish it were possible to bring home to the minds and belief of the general public my conviction regarding re-vaccination, namely, *that it is a sure protection against small-pox.* To ensure this protection, re-vaccination producing some local effect must have been performed after the individual had reached 15 years of age. Cases of variola subsequent to re-vaccination are merely the exceptions that prove the rule; they are more uncommon than second small-pox, and differ also in this way, that whereas the latter are frequently severe, and sometimes fatal, the former are very mild indeed.

Age.—The extremes are alike unfavourable in small-pox, as will be seen by a reference to Table II, in which the percentages of mortality are highest at the beginning and end. Taking the unvaccinated class, amongst whom age is the chief modifying influence, we find the period of minimum mortality to be between the 10th and 20th years of age. Under 5 as many as 70·87 per cent. die, this diminishes to 35·03 per cent. between 10 and 20, and increases gradually to 100 per cent. over 60, but the number in the last category is so small as to possess little value. Amongst the vaccinated the period of minimum mortality is also between 10 and 20, after 20 the increase is proportionately greater than in the

unvaccinated, showing that the protective power of vaccination becomes lessened by the efflux of time. Unvaccinated infants under one year of age rarely recovered.

It is worthy of note that cases of small-pox of the hæmorrhagic type, unfortunately in this epidemic so common in adults, were very seldom found amongst the children.

Sex.—More males than females were treated, the active work of men bringing them in more frequent contact with the contagion: the numbers were 3,377 males and 2,844 females; of the former 680, or 20·13 per cent. died; of the latter 525, or 18·49 per cent., a higher rate amongst males than females. This is the case both in the vaccinated and unvaccinated classes; but, taking age in conjunction with sex, we find that the lower rate of females is limited to adult life. Separating the patients under 20 from those over that age, the statistics are as follows:—

Males under 20—				
Vaccinated admitted	1,210,	died	87	7·8 per cent.
Unvaccinated „	491	„	251	= 51·12 „
Females under 20—				
Vaccinated admitted	1,103,	died	79	= 7·16 per cent.
Unvaccinated „	410	„	225	= 54·87 „
Males over 20—				
Vaccinated admitted	1,506,	died	248	= 16·46 per cent.
Unvaccinated „	170	„	94	= 55·29 „
Females over 20—				
Vaccinated admitted	1,154,	died	153	= 13·35 per cent.
Unvaccinated „	177	„	68	= 38·41 „

What makes the adult male more liable to death when attacked by small-pox than the female?

Primâ facie one would suppose that the female, from her more delicate organisation, would stand the worst chance, but figures show that the reverse is the truth. This effect must be produced by something which lowers the vital powers of men—most probably the mode of life.

It may be that the extra wear and tear undergone by them in the capacity of bread-winners may have something to do with it, but I am inclined to ascribe it to the greater prevalence of dissipated and irregular habits amongst men than women. Nothing unfits one to cope with small-pox more than previous dissipation. The likelihood of an imperfectly vaccinated hard drinker recovering from an attack of this disease is, I believe, small.

Occupation, like sex, influences the mortality both directly

and indirectly; directly, by increasing the number of deaths in the sufferers from the malady; and indirectly, by certain callings bringing those following them in more than average contact with the infectious cause.

Table III, which is a classified list of all occupations, of which we have had more than 10 representatives as patients, exemplifies both influences. The occupations placed in it above the dotted line have amongst the vaccinated a percentage of mortality higher than the average, and those under it, less. By a reference to that table, it will be seen that sedentary employments, contrary to what might have been expected, compare favourably with the others. The greatest mortality occurs in the occupations in which admittedly dissipated habits prevail, or in which the persons employed are exposed to a continuous high temperature. Perhaps the latter circumstance induces the former. Amongst men may be instanced cabmen and blacksmiths, and amongst the women laundresses and cooks, as classes whose occupations have increased the mortality in small-pox. This increase is owing to the large number of hæmorrhagic cases developed under these conditions. In connection with the question of the influence of occupation and habits of life on the mortality of small-pox, much valuable information might be obtained from statistics collected in manufacturing districts compared with others from purely agricultural ones during the same epidemic.

TABLE I.—*Analysis of* 3,555 *Cases in which the number of Marks was noted, with Sex and Age.*

WITHOUT MARKS.

Age.	Males.			Females.			Total.		
	Admitted.	Died.	Per centage.	Admitted.	Died.	Per centage.	Admitted.	Died.	Per centage.
Under 5	85	60	70·58	85	63	74·11	170	123	72·35
Between 5 and 10	100	51	51·	67	37	55·22	167	88	52·69
10 and 20	79	28	35·44	56	21	37·5	135	49	36·25
20 and 30	55	31	56·36	43	23	53·48	98	54	55·10
30 and 40	31	20	64·51	23	12	52·17	54	32	59·25
40 and 50	8	3	38·	4	3	75·	12	6	50·
50 and 60	2	2	100·	5	3	60·	7	5	71·42
Over 60	1	—	—	—	—	—	—	—	—
Total	361	195	54·02	283	162	57·10	644	357	55·43

WITH ONE MARK.

Age.	Males.			Females.			Total.		
	Admitted.	Died.	Percentage.	Admitted.	Died.	Percentage.	Admitted.	Died.	Percentage.
Under 5	5	1	20·	10	3	30·	15	4	26·6
Between 5 and 10	22	4	18·18	16	4	25·	38	8	21·05
10 and 10	107	8	7·47	73	16	13·69	180	18	10·
20 and 30	87	20	22·98	103	20	19·41	190	40	21·05
30 and 40	40	7	17·5	29	6	20·71	69	13	18·98
40 and 50	12	2	16·66	12	3	25·	24	5	20·91
50 and 60	3	1	33·3	5	1	20·	8	2	25·
Over 60	3	1	33·3	2	1	50·	5	2	40·
Total	279	44	15·77	250	48	19·2	92	529	17·39

WITH TWO MARKS.

Under 5	5	2	40·	4	2	50·	9	4	44·4
Between 5 and 10	24	2	8·33	25	2	8·	49	4	8·16
10 and 20	210	11	5·23	175	8	4·57	385	19	4·93
20 and 30	245	44	17·95	167	26	15·56	412	70	19·39
30 and 40	91	22	24·15	65	9	13·84	156	31	19·87
40 and 50	27	4	14·81	14	5	35·71	41	9	21·95
50 and 60	10	2	20·	6	1	16·66	16	3	18·75
Over 60	4	2	50·	3	1	33·3	7	3	42·85
Total	616	89	14·44	459	54	11·76	1075	143	13·29

WITH THREE MARKS.

Under 5	5	2	40·	6	1	16·6	11	3	27·27
Between 5 and 10	25	1	4·	21	—	—	46	1	2·17
10 and 20	157	10	6·36	124	2	1·53	281	12	4·27
20 and 30	130	17	13·07	98	13	13·26	228	30	13·15
30 and 40	38	12	31·57	33	4	12·12	71	16	22 53
40 and 50	19	6	31·57	11	3	27·27	30	9	30·
50 and 60	2	—	—	1	—	—	3	—	—
Over 60	1	—	—	—	—	—	1	—	—
Total	180	48	12·73	294	10	7·85	671	71	10·58

WITH FOUR MARKS.

Under 5	3	—	—	3	—	—	6	—	—
Between 5 and 10	13	—	—	18	1	5·5	31	1	3·22
10 and 20	72	4	5·5	68	5	7·35	140	9	6·42
20 and 30	67	10	14·92	47	1	2·12	114	11	9·64
30 and 40	13	3	23·07	11	3	27·27	24	6	25·
40 and 50	9	—	—	3	—	—	12	—	—
50 and 60	3	1	33·3	4	—	—	7	1	14·28
Over 60	—	—	—	—	—	—	—	—	—
Total	377	18	10·	154	23	6·49	334	28	8·38

WITH FIVE OR MORE MARKS.

Age.	Males.			Females.			Total.		
	Admitted.	Died.	Percentage.	Admitted.	Died.	Percentage.	Admitted.	Died.	Percentage.
Under 5	2	1	50·	1	—	—	3	1	33·3
Between 5 and 10	8	—	—	17	1	5·88	25	1	4.
10 and 20	27	1	3·70	43	5	11·62	70	6	8·57
20 and 30	42	1	2·38	42	2	4·76	84	3	3·57
30 and 40	11	—	—	6	2	33·3	17	2	11·76
40 and 50	2	—	—	1	—	—	3	—	—
50 and 60	—	—	—	—	—	—	—	—	—
Over 60	—	—	—	—	—	—	—	—	—
Total	92	3	3·26	110	10	9·09	212	13	6·13

TABLE II.—*General Summary at Different Ages.*

MALES.

Age.	Without Marks.			With Marks.			Total.		
	Admitted.	Died.	Percentage.	Admitted.	Died.	Percentage.	Admitted.	Died.	Percentage.
Under 5	165	117	70·90	44	16	36·36	209	133	63·75
Between 5 and 10	188	87	46·27	169	12	7·10	357	99	27·74
10 and 20	138	47	34·05	997	59	5·90	1135	106	9·42
20 and 30	96	51	53·12	1015	142	13·99	1111	193	17·37
30 and 40	54	30	55·55	332	69	20·78	386	99	25·67
40 and 50	16	9	56·25	120	28	23·3	136	37	27·94
50 and 60	2	2	100·	28	5	17·85	30	7	23·3
Above 60	2	2	100·	11	4	36·36	13	6	46·15
Total	661	345	52·19	2716	335	12·33	3377	680	20·13

FEMALES.

Under 5	144	102	70·83	39	9	23·07	183	111	60·10
Between 5 and 10	150	81	54·	170	12	7·05	320	93	29·06
10 and 20	116	42	36·20	894	58	6·48	1010	100	9·9
20 and 30	120	41	34·16	791	82	10·36	911	123	13·50
30 and 40	36	17	47·2	245	43	17·95	281	60	21·35
40 and 50	15	6	40·	76	22	28·94	91	28	30·76
50 and 60	6	4	66·6	32	4	12·5	38	8	21·05
Above 60	—	—	—	10	2	20·	10	2	20·
Total	587	293	49·74	2257	232	10·27	2844	525	18·49

TOTAL OF BOTH SEXES.

Age.	Without Marks.			With Marks.			Total.		
	Admitted.	Died	Percentage.	Admitted.	Died.	Percentage.	Admitted.	Died.	Percentage.
Under 5	309	219	70·87	83	25	30·12	392	244	62·24
Between 5 and 10	338	168	49·73	339	24	7·08	677	192	28·36
10 and 20	254	89	35·03	1891	117	6·18	2145	206	9·60
20 and 30	216	92	42·59	1806	224	12·40	2022	316	15·63
30 and 40	90	47	52·2	577	112	19·41	667	159	23·83
40 and 50	31	15	48·38	196	50	25·51	227	65	28·68
50 and 60	8	6	75·	60	9	15·	68	15	22·05
Above 60	2	2	1[illegible]0·	21	6	28·57	23	8	34·78
Total	1248	638	51·12	4973	567	11·40	6221	1205	19·36

Table III.—*Occupations Classified.*

Occupation of males over 20.	Vaccinated.			Unvaccinated	
Trades.	Admitted.	Died.	Percentage.	Admitted.	Died.
Blacksmiths	10	5	50·	—	—
Cabmen	12	6	50·	2	1
Painters	20	6	30·	2	1
Grooms	41	9	21·95	1	—
Barmen, etc.	25	5	20.	3	1
Butchers	21	4	19·04	—	—
Labourers	132	12	18·93	12	7
Bricklayers, etc.	32	6	18·75	2	1
Drapers and Hosiers, etc.	11	2	18·18	2	1
Jewellers	12	2	16·6	2	1
General percentage of mortality in Vaccinated Males over 20 = 16·46.					
Shoemakers	20	3	15·	3	—
Tinmen	14	2	14·29	3	1
Carmen	21	3	14·28	5	4
Tailors	15	2	13·3	3	2
Clerks, etc.	23	3	33·04	1	1
Carpenters, etc.	9	1	11·1	7	2
Gasfitters, etc.	9	1	11·1	1	—
Costermongers, etc.	27	3	11·1	1	1
Porters	67	2	20·44	7	2
Coachmen	11	1	9·09	1	—
Salesmen	14	1	7·14	1	—

Occupation of Females over 20.	Vaccinated.			Unvaccinated.	
Trades.	Admitted.	Died.	Percentage.	Admitted.	Died.
Domestic servants, including Cooks	181	39	21·60	18	11
Laundresses	27	5	18·51	I	—
Sempstresses	72	10	13·8	6	5
General percentage of mortality in Vaccinated Females over 20=13·35.					
Charwomen	20	2	10	4	2

I have thus endeavoured to briefly present to you some of the facts deduced from the analysis of over 6,000 cases of small-pox treated during the present epidemic, one which for severity has not been surpassed since the practice of vaccination was generally introduced. In doing so I have without doubt but repeated what is to many of you an old story. But if, from the figures here adduced, the smallest iota of additional proof is furnished of the immense value of vaccination and re-vaccination as a protection against this most terrible disease, or if one of the many benefits to be derived from leading a temperate life is shown, I will think the time is not wasted which has been given to their collection and arrangement.

ON THE DISTRIBUTION OF ASIATIC CHOLERA IN AFRICA.

By Dr. SMART, C.B., K.H., R.N.,

INSPECTOR-GENERAL OF FLEETS AND HOSPITALS.

[*Read June 12th*, 1872.]

Inquiry has been successfully directed to the history, course, and means of propagation of this disease in Asia, Europe, and America; but hitherto no attempt has been made to collect the scattered facts concerning its visitation of Africa.

The geographical peculiarities of this continent occasion special relations of commerce from without, and of intercourse within, its limits; and by these the spreading of cholera in Africa has been regulated in a very remarkable manner. The great centres of demi-civilisation that fringe its coasts are isolated from each other in a manner unseen elsewhere; and the commerce of these is carried on with widely separated countries, and the sources from which the invasions of cholera can be traced are almost invariably identical with those of commerce.

Thus, Egypt can trace back to Arabia some of its visitations as the sequels of pilgrimages to the holy cities; and Abyssinia may trace hers to the same source; not merely by means of their own pilgrims, but by the transit of those belonging to more remote Mohamedan countries. Through each of these channels the disease has been widely dispersed—in the one instance through Europe to America; and in the other, into the regions of Africa lying south of the great desert of sand with its oases or green islets, analogous to an ocean with its scattered islands. From Egypt, the disease has been widely dispersed, with all the rapidity which modern steam navigation provides: and from Abyssinia with the slow progress of caravans, or the still slower steps of vagrant or nomadic hordes of negroes, who travel

on foot from the interior of Africa to the shores of the Red Sea to cross over to, and to return from, the holy cities of Arabia, carrying amidst them the essential or specific causes of epidemic Asiatic cholera.

Outside the straits of Babel-man-deb there lies a great centre of African commerce, at Berberah; and there an annual fair lasts six months, from November to April, during which the commodities of the Galla country are exchanged for those brought from Southern Arabia and the Persian Gulf. Together with the commodities of Southern Arabia, the cholera has been imported into Berberah, and has been conveyed by returning caravans across country, along the course of the Juba river, to appear again at the eastern coast; and, likewise, by a more inland route, to the Galla country, and thence through southern tribes, until it has been met with by caravans that have started from Zanzibar to trade on the borders of Lake Tanganyika.

Further southward, on the east coast of Africa, lies that great centre of Arab commerce—Zanzibar; which sends its great caravans far into the interior to bring back ivory and slaves, and attracts to its fine harbour vessels from the Persian Gulf, from Cutch, Bombay, Surat, and the entire Malabar coast, to traffic for the contents of the caravans, and the produce of the slave-cultivated lands near the coast. This great commerce is regulated by the monsoons, which blow from the N.E., wafting the ships from Asia, between December and March, or from the S.W., carrying them back again, between June and October. By the same means, the coasters of Zanzibar are carried to and fro on their voyages to the Portuguese settlements at the south, to the Comoros, and to Madagascar, where the natives of Arabia and of India are the chief merchants in every port. Thus, the southward traffic of Zanzibar commences subsequently to the arrival there of the vessels from India, the Persian Gulf, and Arabia.

Everywhere in this direction are found the most striking proofs of the dissemination of cholera in the great stream, and in the smaller channels, of commerce. Epidemics appear in Zanzibar after the arrival of vessels from Asia, and spread along the coast north and south of it, following the traffic of the monsoons with certainty, and falling short with them. From this port the disease has gone south to Mozambique and Quillimane, on the delta of the Zambesi, and it has been carried north to the island of Socotra off Cape Guardafui; while it has been conveyed, stage by stage, from

the coast of Africa to the Comoros, Madagascar, and lately to Bourbon, if not to Mauritius itself, which were wont to receive the disease from southern India.

The southern part of the continent, from Quillimane, in 18°, S. Lat., round by the Cape of Good Hope to Rio Grande, in 11°, N. Lat., has not yet suffered from epidemic cholera. The last-named position is the limit of the course of the disease along the north western shores of Africa; but our ignorance of what occurs in the great fertile track between the White Nile and the sources of the Niger, forbids its being positively asserted that the disease has not reached the west coast; through the tribes of Mohamedan negroes populating that belt, known collectively as *Tocrur*, the pilgrims from which are the most destitute that reach the holy places of Arabia, through Abyssinia and Sennaar.

Retracing our steps to Egypt, of which it was said that its visitations were generally due to the intercourse of pilgrims with Arabia, and casting from it westward, the states of Barca and Tripoli would appear to have been less frequently infected. Still further west, the kingdom of Tunis has presented epidemics, having more frequently relations to those of Algeria than to those of Egypt; while Algeria itself, seems to have been almost entirely dependent on southern Europe for its infection, and Morocco has suffered concurrently with Algeria in most instances, but sometimes with Spain; and in it the disease has extended south westward to Mogadore and Safi on the west coast, and inland to Fez and its southern provinces, from which it has been conveyed by caravans across the desert to the Senegal, and along that coast to the southern position of the Rio Grande already referred to, at the same time as it was stated to prevail in Bonadoo, eight hundred miles from the coast on the upper water shed of the Niger, which courses through the western states of Tocrur or Mohamedan Central Africa. Thus, we find that in this great continent there are several tracks of invasion of cholera; of which, that through Egypt and Abyssinia and the central plains of Africa derives more directly from Arabia, that through Zanzibar, from the Persian Gulf and the western shores of the Indian peninsula, and that of Algeria and Morocco from the south of Europe: all of which have one feature in common, viz., of following the well-known tracks of commerce and human intercourse, progressing in exact ratio with the rapidity or slowness of that intercourse.

After this general survey of the invasions and attacks of cholera in Africa, we may take a more circumstantial view of them in the different regions thus defined.

Arabia was the earliest source of epidemic cholera in Africa. The best authority on the early history of the disease, Dr. John Macpherson, in a paper (*Epidemiological Trans.*, Vol. iii, Part 1), has quoted from the writings of Zacutus Lusitanus, A.D. 1629, that the disease then known in India as *Mordeshi*, "in Mauritania et Arabia est lethalis fere, in quem affectum incidunt Arabes frequenter, qui continuo jusculum esitant frigidum quod *cuscus* vocatur." Accepting this literally, we derive from it a knowledge that among the Arabs and Moors, who used in common one national dish, the same disease existed as that which had become known to the earliest Portuguese settlers at Goa in India, where, at that time, it was likewise attributed to dietetic causes alone, without any recognition of endemic or epidemic characters. It may be inferred that a severe form of sporadic bilious cholera was then known in the Algeria of our day, as well as in Arabia, among people of the same habits of life; and even in our own time there are, in this respect, but few differences between the Bedouins of Arabia and the Kabyles of Algeria. The disease, however, seems to have been unknown at that time in Egypt, as Prosper Alpinus, who wrote his treatise on Egyptian medicine in 1581, after residing two years in that country and six in the Levant solely for medical exploration, makes no allusion whatever to the disease. History is mute on cholera in Egypt up to that date; and therefore, if there be truth in the allusion made by Dr. William Ainslie to "a tradition among the Arabs that the disease was introduced into Arabia some centuries ago from India, and that it travelled over Persia, Syria, and Egypt, *and finally disappeared in the African desert*"; that event must have happened after Alpinus wrote. With this reservation as to period, the tradition is not improbable, but it is involved in mystery.

Dr. Macpherson has proved the true epidemic nature of the disease in India as it prevailed in Goa in June and July 1543, according to D'Orta, who wrote in 1563, and thus the statement of Zacutus, as to its prevalence in Arabia when he wrote in 1629, is not incredible; and that it may soon after that have spread from Arabia into Egypt as an epidemic—a little later than when Alpinus wrote—is not improbable. It must be admitted, however, this is but the foundation of a theory on very insecure paths of medical

history, and that there is no substantial ground for assigning an earlier date than 1831 for the existence in Egypt of the true epidemic form of Asiatic spasmodic cholera, subsequent to its appearance at the festivals of the holy cities of Arabia. The connection between the events of the festival at Mecca and of cholera in Egypt, was clear on that occasion, although it may have been denied on later occasions. Salim-Bey states that only on two occasions, 1831 and 1865, has Egypt been invaded by cholera at the moment of the return of the pilgrims from Mecca.

A certain degree of relationship between the epidemics of Arabia and Egypt, has negative as well as positive confirmation, in the facts connected with the first historical extension of the disease westward from India in 1821, when it followed two tracks; of which, that through Independent Tartary ceased at Orenburg on the Oural, and the other through Persia, ended on the Syrian shore in 1823. On that occasion, neither Arabia nor Egypt suffered.

The second westward invasion left India in 1827, was at Orenburg in 1829, from which it passed into Europe in 1830.

Arabia was infected in 1831, 1835, 1846, 1847, and 1848, and again in 1859, then recurring mildly every year till the next great outbreak in 1865, since when it has again made its annual presence known in milder type.

Egypt has undergone epidemics in 1831, 1837, 1848, 1850, 1855, and 1865. Salim Bey, the representative of Egypt at the congress of 1866, affirmed that of these epidemics those of 1831 and 1865 were the only instances of the epidemic outburst in Egypt subsequent to the festivals in Arabia, and that all of them had occurred between the 4th of June and the 25th of July, which would seem to indicate that certain climatic conditions are concurrent with these epidemic outbursts. But the devastation that arose at the festival of Tantah in June 1848, was preceded by the disease at Mecca in December 1847, and it is reasonable to assume a connection between these facts, as some who had been at Mecca may also have been present at the great assemblage of Mohamedans at Tantah. In 1850, there would appear to have been no cholera in Arabia, but it was present in Algeria and Tunis in 1849, and there also severely, as well as at Malta infected from Tunis, in 1850; consequently, as that was the time when pilgrims passed through Egypt, *en route* from the west towards Arabia, it may be inferred that the disease in Egypt was derived from the west on that occasion as Tripoli was then infected from

Tunis. Again in 1855, Arabia is reported to have been free, but in that eventful year the disease prevailed on the shores of the Mediterranean, from Spain to the Crimea, and an unrestricted intercourse was carried on between Turkey and Egypt, and by its means the disease may have been readily introduced into Egypt.

In 1865, the track of the disease from Arabia lay through Egypt, from which it radiated to the shores of Europe, which would appear to have been the only occasion in which it did so, and this is very remarkable in the history of epidemic diffusion.

If these data be correct and these views be adopted, it follows that we may credit the Asiatic cholera epidemics of Egypt to have been derived from Arabia in 1831, 1848, and 1865; from Tunis in 1850; and from Turkey in 1855.

M. Bricquet asserts that the cholera has never lasted beyond six months in Egypt, and has never become endemic there, which implies that on each occasion of outbreak it has been by a fresh invasion.

The principal determining element of these epidemics appears to have been the movement of the vast bodies of hadjis from and to the holy cities of Arabia, in attending the Kourban Bairam festival, which is moveable between May and November, while the period of outbreak of Egyptian epidemics lies in June and July; and thus we see that the epidemics of 1831, 1848, and 1865 were related to the return from the festival, while that of 1850 occurred previous to or in the passing through Egypt of the hadjis from the west towards Arabia, and the epidemic of 1855 had no relation whatever to Arabia, which was healthy that year. Formerly, the pilgrimage occupied more time, when the western hadjis went to Egypt by slow sailing native craft, or in caravans skirting the Mediterranean shore, when from Egypt the great caravan route lay around the head of the Red Sea and then along the Arabian shore, occupying much time in the transit to and fro. Thus there was a greater interval between the departure from Arabia and the outbreak in Egypt. Since 1850, the extension of commerce by steam in the Mediterranean and in the Red Sea has revolutionised the entire programme of these pilgrimages, by completing in as many weeks what it formerly took as many months to perform; and thus in 1865 the epidemic of May in Arabia was renewed in Egypt in June.

On the other hand, the greater facilities of getting away from the holy places, seem to have curtailed its invasive

force when the disease has been of the merely endemic type, as from 1859 to 1864 inclusive, while it has been effective only when the highest degree of epidemic type has prevailed at the holy places, as in 1865. All the known facts concerning the Egyptian epidemics serve to prove how much the outbursts of cholera are dependent on human intercourse, and especially upon the movements and the rests of large bodies of men ill-provided for travel, decreasing so long as they are actually in motion, and developing with renewed vigour when they come to a halt, whether in crowded cities or temporary encampments; just as in ships a fair side wind speedily extinguishes cholera, but head winds and a slow progress cause it to linger, with tendency to renewed outbursts.

Westward of Egypt, between it and Tunis, in the great syrtis of sandy soil, lie the kingdoms of Barca and Tripoli. The data concerning cholera in these states are very indecisive. When Egypt was visited in 1831, 1837, and 1848, Tripoli is said to have escaped. Its first visitation of 1850 is said to have been from Tunis, when Egypt also appears to have been infected. On that occasion, the disease crept along the Barbary shore, from west to east. In 1849, it had extended from Oran to Tunis; and in the spring of 1850, it advanced again towards the east, from Tunis to Malta and Tripoli, and finally to Egypt.

In 1853, cholera was again seen in Tripoli, and other spots on the Barbary shore; and, according to Dr. Milroy, it was difficult to account for its presence there; as on the one hand Egypt, and on the other Algeria, together with southern Italy and Malta, remained exempt. In 1855, Tripoli suffered later than Egypt, but Tunis was exempt, although Algeria, Sicily, and Malta were infected.

In 1856, Tunis had a slight visitation, Malta then suffering more severely than in the preceding year; but Tripoli on the east and Algiers on the west were free. In 1865, when the great epidemic radiated from Egypt to all the north shore of the Mediterranean, the Barbary shore was untouched by it as far as Algiers, where the infection was traced from Marseilles. On this occasion, Tunis and Morocco observed a strict quarantine towards the pilgrims on their return from Egypt. In 1867, Tunis was again infected, subsequently to Algiers, into which the disease was imported from the south of France in 1866.

Thus, it would appear that the course of cholera in the Barbary States has been more frequently from west to east

than in the contrary direction, and that this applies to Tripoli lying next to Egypt, as well as to Tunis. In 1850, Tripoli was visited after Tunis, and the disease seems to have lurked there till 1853, when it reappeared in epidemic type. In 1855, Tripoli was visited after Egypt, and Tunis suffered the following year. In 1867, Tunis was again infected from Algeria, without extension to Tripoli.

The States of Tunis and Algeria are contiguous, and there has been a relation of time, if not a succession, between their epidemics. Thus, 1834 and 1835 in Algeria, and 1835 at Tunis; 1849 in Algeria, 1850 in Tunis; 1855 in Algeria, 1856 in Tunis; and lastly, 1866 in Algiers, and 1867 in Tunis. In addition to its contiguity to Algeria, into which the disease has been frequently imported from France, Tunis has been open to infection by hadjis returning from Arabia through Egypt, affording it a double liability. Its epidemics of 1850 and 1855 occurred in years of epidemics in Egypt, but it did not suffer when Egypt suffered in 1831, 1837, 1848, and 1865, and it suffered when Egypt did not in 1835, 1856, and 1867.

Hitherto the survey has been over coasts and states that have at different times received the infection of cholera, sometimes from Egypt in the east, and at other times from Algeria in the west; but now we arrive at a region of Africa which has owed all its infections to Europe, so that in the first and second epidemic cycles, from 1831 to 1860, the track of the Asiatic disease into this part of Africa lay through the north of Europe, round by its western countries to the northern Mediterranean shores, and across that sea into Africa. There have been so many and so great peculiarities in the outbursts and extensions of the disease in this geographical region, that as regards its epidemiology we may consider this *western basin of the Mediterranean* as a defined cholera region, in the same sense as the countries on the south the Baltic are regarded as such. It is bounded on the west by Spain and Morocco; on the north by France; south by Algiers and Tunis; and on the east by Italy, Sicily, and Malta. In it the first European epidemic took a reverted course from west to east in 1834, after lingering a year and a half in the peninsula, regaining epidemic intensity there. The second European epidemic passed over the face of the continent in 1848 and 1849, after which there was a general rest, excepting in the south of the Baltic basin, where it became endemic in Silesia and Poland, to spread again over northern and central Europe in 1853. In the western

Mediterranean basin, it began in Spain in 1848, shewed its greatest extent in 1849, and disappeared late in 1850, reappearing in 1853 in such an unaccountable way in the Peninsula that its origin was attributed to an importation from the Havanna into Vigo. From that time it remained in the western basin until 1857, or until the end of 1860, with an intercalated free year, 1858, Spain being the first and last infected country on the shores of the basin. The revival of the disease in 1859-60 was not traceable to any fresh infection from without, and M. Didiot has not hesitated to assign to it a *spontaneous origin* in Spain and Oran in the summer and autumn of 1859, and it is remarkable that while the disease extended through Morocco, Portugal escaped and Gibraltar was free until 1860 the second year of its continuance.

The year 1860, like 1837, was the expiring epoch of an epidemic cycle, and both are distinguished by numerous recrudescences of the disease in its preferred regional haunts, especially so around the western Mediterranean basin, where it lasted from 1848 to 1860, with 1852, 1857, and 1858 free years, so far as is known.

The third great epidemic cycle commenced in 1865, after a rest of four years. The disease radiated into Europe from Egypt. Spain was infected early; Morocco escaped; Algiers received the disease from Marseilles and not from Egypt. The absence of infection direct from Egypt into the Barbary shore, although receiving returning pilgrims, was a marked phenomenon in 1865, which could not be attributed to any other cause than a rigid quarantine exacted by the governments of Tunis and Morocco. It is equally notable that, although escaping by quarantine on the first invasion of the western basin, yet both these countries suffered severely at a later date, Tunis in 1867 and Morocco in 1868-69. This, however, is connected with exemptions of insular positions within the basin, which have exemplified the worth of quarantine in warding off altogether on some, and of postponing on other occasions the invasion of epidemics from the shores of the basin in which they lie.

We are indebted to Drs. Vincent and Collardot, of the hospital staff in Algeria, for an historical account of the epidemics of that country up to 1865, from which they have deduced strong arguments in favour of quarantine from the facts which they adduced. The greater reliance is due to their statements, as at the time of their production they

incurred great risk of official displeasure from opposing the opinions that actuated the policy of their country.

The coast of Algeria extends over six hundred miles. Its western city is Oran, with its port of Mirs-el-Keber, holding much intercourse with Spain as well as with France, and forming the stragetic position from which the fortified towns towards the Morocco frontier obtain their reinforcements and supplies. Its eastern port is Bona, near the Tunisian frontiers, having at about thirty miles inland the important fortress of Constantine. Algiers itself lies midway, not easily to be forgotten by those who have looked on it from the sea, rising in pyramidal shape, white and glittering in the sunshine, at the foot of high hills, clothed with verdure to their summits, and their slopes dotted with charming villas. These three ports are the points at which cholera has invaded the country, and has penetrated into the interior.

Drs. V. and C. have shown that the *first* epidemic was imported from Spain into Oran in 1834, and from Toulon and Marseilles into Algiers and Bona in 1835.

The *second* epidemic of 1837 was carried to Bona among troops from Marseilles, and thence to Constantine, then undergoing a siege. After this the colony was exempt for twelve years.

The *third* epidemic was imported from Marseilles in 1849 by a mail steamer, and was carried thence by marching columns to Milianah, Orleans Ville, and Tenez. Later in the same year, Oran was infected direct from Marseilles. In July 1850, the disease broke out again in Algiers, after the arrival of a pilgrim vessel that had touched at Malta, then in a severe epidemic; but shortly after that an infected regiment arrived from Aumàle in the interior, proving that the invasion of 1849 was not yet extinct. In 1851, the disease spreading westward, reached Tlemcen and Ouchda on the Morocco frontier, and visited Oran from the interior, ceasing entirely that year. There was then a rest of eight years.

The *fourth* epidemic began in 1859, during the war between Spain and Morocco, both of which were severely visited. It first showed itself in the French territory in the camp of observation on the Kyss, and later at Oran on the coast, where it was believed to have been brought from Spain; but the three armies were affected simultaneously. From Oran it was conveyed by returning detachments to Algiers later, and lingered in the surrounding country be-

hind the hills, and was reintroduced into the city in August 1860, among troops from Arba in the Mitidjah plain, and prevailed chiefly in barracks and hospitals.

There was then a rest through four years. The fifth epidemic was in 1865, introduced from Marseilles among a corps of hospital orderlies, by whom it was dispersed wherever they were distributed. It extended into the country.

Up to that time the lazaret had been in the port of Algiers, but then an altered policy being adopted, a new quarantine establishment was formed at Sidi-Ferruch, seventeen miles west of Algiers, at which an infected vessel was admitted in 1866, and subsequently to that the disease made itself again felt within the city.

The whole history of cholera in Algeria is of great interest to military epidemiology, displaying as it does frequent outbursts of the disease after the arrival of infected troops from France; the formation of centres of infection in crowded hospitals into which the first cases were received; the extension from thence into barracks and military prisons, aud among the civil population; while on the other hand, it displays striking instances of modification of epidemics by evacuation of their strongholds. It is certain that in Algeria the disease has been carried on in every direction, even into the oases of the desert, in the train of armies and regiments; and at the sieges of Constantine in 1837, and of Zatcha in 1849, martial successes were delayed and compromised by the inopportune arrival of infected columns from the coast into previously healthy camps. Again, frequent instances are shown in which cholera has broken out on the march, among troops apparently healthy on departure, but bearing among them the causes of the disease from infected localities.

Cholera was introduced into Algeria from Europe, by troops, in 1834-35, in 1837, in 1859-60, and in 1865, by passengers in mail steamers in 1849, and in 1854-55, and by a pilgrim vessel from Malta in 1850. A strongly marked feature of cholera in Algeria cannot be passed unnoticed here, its subsidence during the heavy rainfalls that occurred in November and December, and its reappearance in epidemic type in the next spring or early summer, as in 1834-35, in 1849, 1850, and 1851, in 1854-55, in 1859-60, and again in 1865-66, which exemplify the relation of cholera epidemics to meteorological conditions.

As the epidemics of Algeria have borne a very general

relationship to those of the south of France, so those of Morocco have borne a marked affinity to those of Spain.

Less is known of the disease in Morocco than in Algeria, but it would appear that in 1837 and in 1865, Morocco escaped when Spain suffered, and in 1868 Morocco suffered when Spain was free; but in 1834, 1849, 1854, 1859, and 1860, both were simultaneously in epidemics.

In 1865, a very rigid quarantine was enforced with apparent present success; but in 1868, after a year of freedom from disease in Spain, it broke out with great epidemic force in Morocco; and, for the first time known, it was carried from Fez across the great desert to the banks of the Senegal, from which it extended to those of the Gambia and Rio Grande in 1868-69.

The epidemics of the east coast of Africa were next reviewed. Abyssinia has a very constant intercourse with Arabia during the festivals. According to Dr. Blane, it has suffered twice. In 1856, when Arabia and Egypt would appear to have been free, although both were suffering in the preceding year. Again in 1865, when Dr. Blane left it behind him on the coast, where it seemed to have come from Arabia, and found it again at Kassala, two hundred miles inland, where it had previously raged, and in the mountainous central province of Tigre, south of Kassala. This leads to the inference that Abyssinia was in reality undergoing an epidemic in its very centre at as early a date as that of the great epidemic outburst in Arabia in 1865. Taking this with three other regional facts—firstly, that cholera was present late in 1864 at Sana, and other places in south Arabia, as averred by Consul Raby; and secondly, that the captains of the ships *Persia* and *Northwind*, which ships were charged with having imported the disease into Djeddah early in March 1865, affirmed that the disease broke out on board them after leaving Makallah; and thirdly, the extension of disease early in 1865, from the plain of Berbera along the course of the river Juba; there is left by this combination but little if any room for doubt that the whole region was deeply infected previous to the arrival of pilgrims from India and Java.

This Abyssinian epidemic bore early in 1865 such relationship to the Kourban-Bairam epidemic of Arabia, that it was advocated as its forerunner by Dr. Bartoletti, the representative of Turkey at the Sanitary Congress of 1866. It may, however, be accepted as an indication of pre-existing and widely diffused choleraic infection of the regions border-

ing the mouth of the Red Sea, previous to its outbreak with epidemic intensity at Mecca in 1865. Dr. Blane and Mr. Rassam have given their personal knowledge that while cholera was raging in the interior at Kassala and in Tigre province in September 1865, there was an outbreak of it at Massowah on the coast, which was probably brought there by pilgrims from Arabia. That double epidemic having two sources was not extinct in the interior until late in 1866, and the disease was unheard of by the British expedition of 1867-8.

Abyssinia is separated by a wide extent of pastoral country, inhabited by numerous tribes constantly at war with each other, from the territories of the Sultan of Zanzibar, whose chief town in the island of that name is the great emporium of eastern equatorial Africa. It has great commerce by sea regulated by the monsoons. Thus, Zanzibar is directly liable to infection from Asia, while its southern commerce by native vessels, etc., to the Portuguese dominions and Madagascar, with the intervening Comoro Islands, renders it the disseminator of cholera into those countries.

On the other hand, it has a very extensive inland commerce by caravans as far as the shores of the Nile lakes; while from the northern tributary ports, Brada, Larnos, and Magadoxa, caravan connection is kept up along the line of the river Juba, with Berbera on the Gulf of Aden, to which the people of India, Arabia, Abyssinia, and the interior of Africa are drawn to a great annual fair of six months duration, from November to April, while the northerly monsoon lasts.

These topographical and meteorological facts have a most marked influence on the history of cholera in the region. This Society is indebted to Dr. Christie, physician to the Sultan of Zanzibar, for a very lucid description of the four epidemics of cholera that have visited the east coast of Africa in 1837, 1858, 1865, and 1869. The first of those dates was that of the remarkable return of the disease in various parts of Europe, America, and Africa, that had been severely visited during its continuance since 1832. Syria and Egypt were severely visited in 1837, but Hedjaz would appear to have been free. Dr. Christie states that it was first heard of at the northern ports, and it extended to Zanzibar, where it was remembered as the AFRICAN CHOLERA, on account of its attacking almost entirely the Negro population, to whom it was a new disease, while the Indians es-

caped. The second epidemic of 1858, like the former, was relatively late in the general run of the epidemic cycle, near to its cessation, and it also was first known at the northern ports, from which it extended south. It was preceded by an epidemic of variola, and exhausted itself on the Negroes, affecting Arabs more than in 1837, and leaving Indians and Europeans almost unscathed. It extended as far south as Mozambique in the Portuguese settlements.

These two epidemics were purely regulated by the prevailing monsoon, and may have been brought in the course of commerce from western India; although it must be remembered that on the last occasion, at least, the Gulf of Oman and the southern shore of Arabia were suffering from the disease, and may have imparted it.

The third epidemic, of 1865, did not reach Zanzibar, owing to its coming at the end of the northerly monsoon. Like the two previous ones, it first displayed itself in the north-ports, but it was traced back to Berbera, on the Gulf of Aden, from which it was conveyed along the caravan route to Gananah and Barderah, on the river Juba, from which it extended to the coast, and spread towards the north, as well as towards the south, dependent on the commerce of the season of variable winds.

Owing to the lateness of the season when it reached the coast, April, it did not extend nearer to Zanzibar than two hundred miles; but Dr. Christie mentions that from Melinda it struck off inland to Unyanyembi, and still more distant points. He states also, that the native traders attributed its importation into Berbera from the Red Sea coast, which, if true, confirms the belief in the pre-existence of cholera there in 1864, as stated by Consul Raby; but it is equally probable that it was imported direct from the Persian Gulf or from India to Berbera, late in 1864, and at about the same time, the early part of the N.E. monsoon, into the coast south of Cape Guardafui.

Concerning the termination of this epidemic, it would be of great interest to ascertain in what part of the interior it was last heard of, and whether it died out or went onwards from station to station, as bearing on the question whether it might possibly have had an African continuation into the epidemic of 1869-70, which was brought from the interior to the coast.

The epidemic of 1869-70 had as its great distinguishing feature that its original source was hidden in obscurity, which has led Dr. Christie to suggest that, like that of

1865, it might have been brought from Berbera, or that it may have extended from Abyssinia through the Galla country, or have travelled from Central Africa across the Nile, somewhere near Gondokoro, in 4°, N. lat. The former suggestion may be dismissed at once, on the ground of there being no data of cholera at Berbera after 1865, which would have been heard of on the Juba from the returning native traders, if any such had existed. This is certain, that it was first heard of much further to the west, in the country between Kili-manjaro and the Victoria Nyanza, and that it was there reported to have been brought from the north, out of the Galla country.

The probability of its descent from the Abyssinian epidemic of 1866 is manifest, as the army of Theodore was greatly recruited from the Gallas, and his slave markets were also much supplied by that race dwelling immediately south of Abyssinia. Thus, the disease, which disappeared from the central provinces of Abyssinia late in 1866, may have travelled into the Galla country, and rested there for a time, or have gone slowly south until, according to Dr. Christie, the Galla Borani imparted it to the Somul Gurras from whom the Wamasai received the infection late in 1868, and gave it to an Arab caravan from Zanzibar, in Laikepya, near the great lakes, about the middle of 1869. There is a distance eight hundred or nine hundred miles between Gondar and Laikepya, and according to the known law of progress of cholera, relatively to that of mankind in any given position, the interval of time is not disproportionate to the distance, where *on foot* is the ordinary mode of travelling in a country inhabited by independent warlike tribes, who rarely communicate except by feuds and raids rather than in pursuit of commerce.

The last suggestion of Dr. Christie is that the epidemic had burst out from the centre of Africa, crossing the Nile at or about Gondokoro, ascending by the course of the river, as it were, to strike off eastward in the neighbourhood of its great lakes. Admitting the possibility of this, it is apparently a far more devious track than that through Abyssinia, and it assumes the pre-existence of cholera in the great fertile belt of Central Africa, stretching from the banks of the White Nile to the upper course of the Niger.

As there is much that is attractive in this theory, it may be examined by what little light we possess. Setting aside the possibility of the disease being carried from Tripoli, Tunis, and Algiers across the Sahara into the central plains,

we have to see if there can have been any relation between the Arabian epidemic of 1865, this east coast of Africa epidemic of 1869, and an epidemic at Bonadoo, eight hundred miles from the mouth of the Gambia, on the west coast of Africa in 1869.

If there were such relationship, it could only have been established and maintained through the agency of the Negro races that inhabit the land, who are designated by the Caucasian Mahomedans, *Takroories of Soudan.* Major Denham and Lieut. Clapperton brought with them from Sokata or Tocrur, and published in the appendix to the account of their first journey, "a translation of a geographical and historical account of the kingdom of Tocrur, now under the control of Sultan Mohammed Bello of Houssa, extracted from a larger work prepared by the said Sultan."

From that authentic source we learn that Tocrur includes all the countries from Nubia and Sennaar in the east, to Houssa, on the Niger in the west. That from the whole of that immense region the Mohamedan Negroes make annual pilgrimages on foot.

Dr. Salim Bey, representative of Egypt at the Sanitary Congress of 1866, defined the ports of Kosseir, Souakin, and Massowah as those of transport for the Takroories. All authorities unite in describing these as the most abject of all the pilgrims, as begging the necessaries of life from the rich caravans, their destitution being something almost inconceivable by us. It was in the camp of these wretched negroes that the Arabian epidemic of 1865 burst out, and it would seem highly probable that, infected thus deeply, they may have carried back with them into Africa the seeds of the disease, and have dispersed along their track, or have lighted up among the Negro races in their homes, the disease of which, in its most fatal form, they have the very highest degree of susceptibility.

If it be conceded that cholera may have obtained in this or in any other way its footing in Nigritia, we cannot hesitate to believe in its wide extension in all directions until checked by the sea shore or by uninhabitable lands; although that extension would be a slow one, where progression on foot is the general mode of travelling. Thus the disease would readily enough have taken up four years in finding its way from Arabia to Bonadoo, on the water-shed of the Niger, and to Laikepya, on that of the Nile. Whichever of these theories proposed by Dr. Christie to account for the presence of cholera at Laikepya in 1869 be the right one,

it may be inferred that that epidemic was an offset from the Arabian of 1865, which travelled from the western shore of the Red Sea by a southerly track inland to Zanzibar, and was carried inland again from the coast to a more southern point than before; and was conveyed by sea northwards from Zanzibar to Socotra, southward to the Zambesi and eastward to the Comoro Islands, Madagascar, and Mauritius.

Of the four visitations of cholera to the east coast of Africa, that of 1865 is the only one that happened in the same year with the outburst of a pandemic from the confines of Asia, bearing an evident relation to the new track of cholera across the Indian Ocean, up the Red Sea, and over the Mediterranean into southern Europe. It is noticeable that while the Red Sea track was being made sure and easy by increased facilities of human intercourse, the other was being rendered comparatively abortive through the converse conditions, viz., the falling short of the north eastern monsoon, and the consequent suspension of traffic down the African coast.

It is a curious coincidence that the epidemics of the east coast in 1837, 1858-9, and 1869-70, all fell towards the end of the pandemic cycles to which they severally belonged, just previous to long periods of rest from the disease.

The more frequent route of cholera to Zanzibar has been the oceanic, by means of native vessels, which have conveyed it up and down the coast, obeying the monsoons or trade winds of the Indian Ocean. The traffic between Africa and Madagascar and the intervening islands being also regulated by the same atmospheric currents.

Quarantine at Mozambique appears to have kept that place free in 1869, but in May 1870 it was introduced by native craft, and it spread from thence to Ibo north of it, and Quillimaine far south of it on the coast of Africa in June 1871. In August 1870, it appeared in Madagascar from Mozambique; and in January 1871, it was present in Mauritius, although quarantine with Madagascar had been in force since the beginning of December, at first for fourteen and then for twenty-one days.

Dr. Smart recapitulated the main points of his paper as follows:—

Africa from its geographical features and social conditions possesses four epidemiological regions.

1. Egypt and Tripoli, generally infected from Arabia, but not allied on every occasion.

II. The western basin of the Mediterranean, so far as Tunis, Algeria, and Morocco form part of it, generally from Europe.

III. The west coast south of the Sahara, from Morocco.

IV. The east coast, from Abyssinia to the Portuguese settlements, from Arabia, Persian Gulf, and India.

Egypt, next neighbour to Arabia, has not suffered invariably when Arabia has done so. Its visitations have been of short duration. It has only on one occasion been the source of European infection, in 1865; and, on the other hand, it would appear to have been infected from Europe in 1854.

Tripoli, although not always involved in the epidemics of Egypt, as in 1847, 1848, and 1854, had had its visitations concurrently with others of that country, and also of Tunis.

The western basin of the Mediterranean, except in 1865, when it received its infection from Egypt, has participated in the epidemics of northern Europe, imported into its Atlantic states. But in 1853, and again in 1859, its epidemics are said by some to have had regional origin and growth in the Spanish Peninsula. Algeria and Morocco have been usually indebted to the European shore for the "contagium" of cholera, the former generally to France, except in 1834 and 1859, when it was so to Spain, and the latter to Spain usually. Morocco was free in 1865 when Spain was severely visited; but this was reversed in 1868, Spain being then free and Morocco diseased, having relighted the epidemic that had been present in Algeria in 1866-67, and in Tunis in the latter year.

Tunis has generally followed on Algeria after a year's interval. In 1859-60 it escaped when Algeria was infected. Egypt also was then untouched, Arabia being infected.

Both Morocco and Tunis excluded cholera by sea-quarantine in 1865, but they failed later on their land frontiers. The west coast of Africa was visited for the first and only time in 1868-9, when the disease appeared to have been carried from Fez, across the Sahara, to an upper trading station on the river Senegal, by trading caravans. It then extended to the Gambia and Rio Grande through the inland stations, and was at the same time present in the state of Bonadoo, eight hundred miles inland, between the water-sheds of the Niger and Gambia.

The east coast of Africa, from Abyssinia to the Portuguese settlements, has had a series of epidemics entirely apart from those of Egypt. Abyssinia is in constant communica-

tion with Arabia; and during the season of the N.E. monsoon, from December to March inclusive, Zanzibar has many arrivals from India, the Persian Gulf, and Arabia, from either of which cholera might be conveyed to it, to be spread from it southward, by means of the native trading craft.

The first visitation was in 1836-37, affecting almost exclusively the Negro race. It crept along the coast north of Zanzibar, and from that port to the station south of it, remaining only a few weeks in Zanzibar.

The second visitation was in 1856. It was confined to Abyssinia. Arabia had suffered in the two preceding years, and Zanzibar was undergoing a heavy loss by small-pox in the same year.

The third visitation fell in 1858-59. Whilst Arabia was in an epidemic from Muscat to Djeddah, the disease appeared early in the monsoon in the ports south of Guardafui, and extended to Mozambique with great epidemic force.

The fourth visitation of 1865, the year of the last great epidemic that spread into Europe from Arabia, the disease showed itself very early in the year in Abyssinia, and at the fair of Berbera, on the south shore of the Gulf of Aden. From this latter place, it was conveyed by caravans to the coast of the Indian Ocean; but being late in the monsoon season, it did not extend to Zanzibar.

The fifth and last visitation of Zanzibar in 1869-70 has been traced by Dr. Christie as far as the Galla country immediately south of Abyssinia, and would appear to have been the extension of the great epidemic of 1865, through Abyssinia into the Galla country, travelling slowly southward among pastoral tribes. It seems not impossible that it may have been conveyed into Africa by the Negro pilgrims of Soudan, called *the Takroories or people of Tocrur*, which embraces the fertile plains from the Nile to the Niger. This race was the first to suffer from the epidemic in Arabia, and they may have carried it home with them into the centre of Africa, from which it may have extended westward to Bonadoo by the course of the Niger; and southward by the Nile to the place where the Arab caravans from Zanzibar fell in with it, and took it back with them to that emporium, from which it was carried again inland by caravans that followed a more southerly direction, and northward by sea to Socotra. In 1870, it spread south as far as Quillimane, and seaward to the Comoro Islands and Mada-

gascar, and from it to Mauritius, an island that had on all previous occasions been infected direct from India, or had grown its own epidemics of cholera.

A remarkable feature of the epidemics of the east coast of Africa is that they have, with the exception of that of 1865, arisen in the apogee of the great epidemic cycles when about to cease beyond the limits of Asia, and not when first issuing forth from them, which is strange, if we consider its direct intercourse with India. The history of cholera in Africa bears very strong evidence to the theory that its spread is entirely regulated by human intercourse, and timed relatively to the rapidity or slowness of that intercourse. Africa in this respect presenting the very antithesis of what is shown where mankind travel by steam power, consuming years in effecting as much as is there completed in months.

In the African epidemics the native Negro races have suffered incomparably more severely than the Arabs, Indians, or Europeans present with them. On the Mediterranean shores of Africa there have been repeated instances of the abortive introduction of the "contagium" of cholera; and others, where epidemic intensity has speedily followed on apparently trivial importations. In Algeria, the outbreaks have always followed on a history of importation, and they have been manifested in crowded hospitals, prisons, and barracks, and in camps; and evacuation of these, wherever it has been applied to such foci, has been invariably successful in cutting short the epidemic.

Ample proofs have been afforded of the efficacy of strict quarantine in excluding cholera on the seaboard, although it has appeared in parts thus temporarily exempted, after suspicion had ceased and quarantine was relaxed, being introduced by a slower land intercourse from neighbouring infected states. The great desert itself seems to have been crossed on its western border by a caravan that retained its infection to the Senegal river. And in East Africa, there was in 1869 a remarkable epidemic that appeared to have dwelt an indefinite time among the tribes of the interior. The disease has not extended on the East Coast lower than 18 degs. south latitude, and on the West Coast than 5 degs. north latitude.

ADDRESS AT THE OPENING OF SESSION, 1872-73.

By the President, ROBERT LAWSON, Esq.,

INSPECTOR-GENERAL OF HOSPITALS, ROYAL ARMY.

[*Read November 13th*, 1872.]

In meeting the members of the Epidemiological Society on this occasion, I will endeavour to lay before them a short outline of the epidemics which have shown themselves in various parts of the world since the opening of last session, so far as the materials in my possession will admit. But before entering upon this, it may be well to offer a few remarks on the working of the society.

The study of the epidemiology of the time involves much labour in the collection and arrangement of facts, and, when these are brought together, it requires a person of much experience in dealing with such facts to avoid errors in generalising from them; and even when an effort in this direction has proved successful, the result has seldom any immediate bearing on the ordinary practice of medicine. As the great bulk of our professional brethren have to depend on their daily exertions to procure a livelihood, it is not astonishing that they should occupy themselves with what more concerns their immediate requirements, and that questions of practice should absorb their energies, and leave them little time or inclination to pursue more abstract inquiries. For these reasons, our society has never embraced a large number of members, nor have our meetings, as a rule, had large attendance; neither do I think that, from the nature of subjects which engage our attention, important as they are, we can ever anticipate results very different.

But can nothing be done to render our meetings more attractive, to those at least who enter into the spirit of the subject; and, what is of much more importance, to clear away those differences of interpretation of the same facts

which form so prominent a feature between the different schools of epidemiologists, and which impede our course so greatly in arriving at any common ground of assent. To give an illustration, it is common in this country, for instance, for an epidemic of cholera to be preceded, for some time, by cases varying in character from the slightest form of bilious cholera to others presenting the most marked malignancy, and indistinguishable by any symptom that we can indicate from those met with in the middle of the severest epidemic. Those who consider epidemic cholera imported, discard such cases unless they can affiliate them to some previous case which answers their definition of the epidemic form. Many, on the contrary, who hesitate to admit the evidence adduced in favour of importation as conclusive, point to such cases as indicating the presence and operation of causes sufficient to induce the form of disease subsequently met with, previous to any proof of importation, and hence they deny the necessity of assuming it to explain the facts.

It is clear any number of epidemics may be described, and each party may explain the facts in its own manner; but, until they can agree as to the light in which these sporadic cases are to be viewed, they but waste their energies in opposing each other, and do not contribute anything to settle the essential difference between them, without which no real advance can be made on either side. Other questions of a similar nature will occur to all who are familiar with the current doctrines of the day. Now it appears to me to be an especial function of this society to narrow such questions to the points at issue, and if sufficient notice of them were given to afford all time to prepare for the discussion, though the members may not always agree to one conclusion, I cannot think that the thorough examination the subject would be submitted to, could fail in bringing home to both parties any insufficiency of proof or illogical argument which they had previously employed, and thus tend to the end desired. On the other hand, were the discussions of the society on such points to attain even a moderate success, they would exercise an influence out of doors which they do not possess at present, and could not fail to ensure a much larger attendance when anything of interest was to come on.

To ensure such results, however, it is necessary members should spare no trouble, either themselves or by inducing the co-operation of friends competent for the work, to provide papers suitable for discussion, whether from the origi-

nality of the views submitted, or which dealing with doubtful or diverging opinions current at the time, should endeavour to remove or explain the points of difference between them; bearing in mind that a mere reiteration of opinion on a disputed point, which does not elucidate the subject matter of dispute, is comparatively unprofitable.

Last year I made a few remarks on the epidemics which had been met with in various parts of the world during 1870-71, and brought them down to about the middle of the latter year. This sketch was confessedly very imperfect, as all such attempts must be while our means of acquiring information remain so defective as at present; but, nevertheless, they are of value in the present state of epidemiology in presenting a view of the extent over which epidemic influences have operated, without which we cannot expect to make any real advance in the laws these follow, or still more of the causes on which these laws depend. On this occasion, I will continue the outline to the present time.

The first I will allude to is small-pox, the epidemic of which, now passing away, has been the most serious experienced in this country since the commencement of the registration of deaths in 1838 at least, if not, as some aver, the most severe during the present century. The disease has shown itself very extensively since 1868, and with remarkable severity in every quarter of the globe.

From the facts mentioned by me last year, it appears that, in 1868, a severe epidemic of small-pox was experienced in the Madras Presidency, which caused 34,330 deaths; in 1869, those fell to 18,000, showing a great reduction in the activity of the causes of the disease. In the latter part of 1868 and the first half of 1869, small-pox was very prevalent in the Bombay Presidency; and in 1869, it became very fatal along the Valley of the Ganges, in the Central Provinces, in Oude, the North West Provinces, and Punjaub, through all Hindostan, in short, up to the Himalayas, which was not affected in 1868. In the Punjaub alone the deaths from small-pox were 53,195, nearly six times more numerous than those caused by cholera the same year. Whether the disease proceeded beyond the Himalayas at this time I have no information.

Early in 1870, small-pox showed itself with unusual severity in the south of France, and, somewhat later, at Paris, and, during the fourth quarter, became active in England, in Holland, and at Leipzig, and also at Berlin

either then or early in 1871. In Scotland there were but 74 deaths from small-pox in the eight large towns in 1870, but in 1871 they amounted to 880. The increased frequency of the disease was obvious in every quarter, though it was in the fourth only that it acquired its full developement. While pursuing this course, small-pox became epidemic in the island of Teneriffe, in August 1870, on the one hand; and in Japan, on the other, in December of that year, there was a very severe outbreak, though at what time it commenced I have not ascertained.

On tracing the progress of the epidemic in England, it is found that, from its commencement in the fourth quarter of 1870, the deaths increased to the second of 1871, when they were 7,012. In the third quarter they fell to 4,612, but rose considerably the fourth, and in the first quarter of 1872 amounted to 7,720, since which they have gradually declined. On looking for the cause of this, it appears that in the early part of 1871, though there were traces of small-pox over nearly all the country, yet it had become developed into an epidemic in certain localities only, such as London, Liverpool, the mining districts of Durham, and south Wales, and in these it diminished in the third quarter. Other places and districts, which, up to this time, had been but slightly affected, though interspersed among those which had suffered severely, then began to display the force of the epidemic, and the general mortality again rose. This feature of the epidemic, so clearly developed in England, was met with on a more extended scale in the progress of the disease over Europe, and in the eastern portion of North America. Though it is obvious from the facts given above, that the epidemic influence was in operation in England and Northern Germany in the end of 1870, in many places on the continent small-pox did not present much activity until the latter part of 1871, or even the beginning of 1872, as at St. Petersburg; and in the northern Atlantic States of North America, and in Canada, the same was observed.

These remarks apply to the wave of small-pox seen in the Madras Presidency in 1868, and which in the present year seems disappearing in the north of Europe and America, but besides, there have been numerous outbreaks of this disease over the world, of marked severity, though the facts concerning them are not yet sufficiently known to permit of their being arranged in a similar manner. Among these may be noticed the severe epidemic mentioned by Living-

stone between Ujigi and the coast early in 1871, while on the west of Africa the Gold Coast suffered severely, and, up to November, 500 deaths are said to have been caused by the disease at Brass town in the delta of the Niger. At Trinidad in the West Indies, a severe epidemic commenced about November, 1871, culminated about January, and since decreased; but the disease was very prevalent in Venezuela some months afterwards. Small-pox was severe in Bushire in March and April, 1871, and later there were cases in Ceylon and the Straits settlements, and there was a severe epidemic of it in almost every district of South of India in the early months of the present year (1872). In 1872, small-pox was prevailing at San Francisco in April, and persons recently from that were attacked in Honolulu, in Wellington in New Zealand, and in Melbourne; but in the two latter the disease had not spread up to the date of the last information. A little before, a person from England had arrived at Wellington, labouring under small-pox. There was a severe outbreak at Santiago and Valparaiso, in Chili, about June, which was reported in September as declining, but the disease was then said to be extending along the coast to the northwards, towards Callao in Peru. Small-pox was also occurring in Jamaica in August, and had been there for some time previously. In Bombay this disease had been very frequent since the end of February. In the Mediterranean there was an increase in the disease at Alexandria, in May, 1872; there were slight indications of an increase at Malta and Gibraltar, about the same time, and in Algeria the disease seems to have been prevalent among some of the Arab tribes during the summer. In Vienna, too, small-pox seems to have been remarkably prevalent from the end of June at least, how much before that I have no information.

The striking feature of the recent epidemic has been the frequency with which the hæmorrhagic form has been met with, and the great extent over which it has shown itself. Not only have hæmorrhagic cases presented themselves in this country at many points, but also on the Continent, and we hear of them in Trinidad, in the United States, and in Canada as well. Effects so diffused must be brought about by a factor of equally extensive operation, yet as they are not met with in all epidemics of small-pox, this factor must be distinct from those others which suffice for the development of ordinary epidemics of this disease. Whether it be in any way connected with those influences which have led

to cerebro-spinal fever of late years, is a question for farther observation and enquiry.

The mortality from measles in England and Wales fell from 1868 to 1870, and during that year and 1871, remained without material alteration, though there were local outbreaks in various places of considerable severity. Early this year these continued to show themselves, though without producing much increase in the general mortality of the country. Sunderland, which had suffered in the fourth quarter of 1871, continued to show a high mortality from that cause in the first of 1872. Oldham, also, was suffering from the same cause in the first quarter, and Liverpool and Bradford also presented high rates in the second, and London and Portsmouth the same to a minor extent, while in the vicinity of both there have been several local outbreaks. In Dublin the deaths from measles were few in 1871, but in the second and third quarters of the present year the number was considerable, and the frequency of the disease in several other localities in Ireland was noticed in the former period. In Scotland, in the larger towns, measles attained their greatest force in June, 1871, when the deaths amounted to 275, by October they had diminished to 40, in December they were 69, since which they have diminished again, and now stand at less than half that number. During 1871, this disease prevailed with greater severity in Glasgow, where the mortality from it alone, on the whole year, amounted to 20 in 10,000 living. At Edinburgh, Aberdeen, Paisley, and Greenock, the mortality for the same period varied from 12 to 9, while it was 1½ only at Dundee, and still less at Perth. Since April, however, of the present year, about 58 per cent of the deaths from measles in the Eight Towns, have occurred in Dundee alone.

With reference to such apparent anomalies in the manifestation of epidemics, Dr. Stark remarks in the supplementary report to the monthly and quarterly Returns for Scotland for 1871, "A very slight examination of Table xii suffices to show that the same epidemic disease has not been always the most fatal in each of the Eight Towns. The singular fluctuations in the proportions of deaths caused from time to time in the various towns by such diseases as measles, scarlatina, and hooping cough, have been repeatedly pointed out, and have even seemed to justify the enunciation of a general law affecting these disorders of childhood, viz., 'that while in any of our towns an epidemic disease prevails very extensively, among the youthful population, other epidemic dis-

orders for the most part become less frequent or less fatal.' There can be little doubt that such a general law exists, for it is merely the expression of statistical facts deduced from the sure basis of large numbers, and repeatedly verified. It must not, however, be inferred that the existence of such a law can always be illustrated by the record of a single year's mortality in a town which may happen to have suffered severely from epidemic diseases; for the mere annual return may include the records of several successive and equally serious epidemics, say of scarlatina, of measles, of hooping cough, and of small-pox. During each of these successive epidemics, all others may have fallen into abeyance, so that no two of them can be said to have co-existed; and at the end of a year an annual return may show that each disease has proved fatal to more than an average number of children. It is obvious that in such a case it is from the monthly, or short period returns, that illustration of the law above referred to must be sought" (page 15.) A proper appreciation of these peculiarities is of great importance in the study of epidemics. As several now present may remember, I have on various occasions drawn attention to similar relations between other diseases, and, have been induced to quote Dr. Stark so fully now, to bring independent testimony in favour of these views, and to claim for them an extension to other epidemics than those he mentions, and without limitation as to age.

I have very few notes of the prevalence of measles out of this country. An epidemic mentioned last year as having existed at the Cape during the first half of 1871, seems to have continued till November. In Berlin the mortality from measles during 1871 was 2.6 in 10,000.

The severe epidemic of scarlatina, which culminated in England and Wales in 1870, declined rapidly in 1871, and during the portion of the present year which has elapsed, it has gone still lower. Similar results have been met with both in Ireland and Scotland. With the general decrease in the numbers for the whole country, however, this disease, as we have seen with others, manifested itself in limited localities with considerable energy, from time to time. Thus it was unusually prevalent in Oldham and Sunderland in the first quarter of 1872; in the second in Liverpool, and much more in Bradford, where the mortality was in the annual ratio of 27.6 in 10,000; and Burnley, Dukinfield, and Todmorden were also attacked severely, besides several smaller towns and villages. In the third quarter there were

204 deaths from scarlatina in Oldham, or in the annual ratio of 97 per 10,000, and in the next four weeks of October there were another 102 deaths. In Birmingham there were 122 deaths in the third quarter, or 14 per 10,000, but there, as at Oldham, those in the next four weeks amounted to 92; and in Manchester, in the latter period, there were 57 deaths. In Scotland, both Glasgow and Leith showed a considerable increase in the mortality from scarlatina in September, while in Edinburgh a previous high death-rate was considerably reduced in that month. In Dublin and the other large towns of Ireland, there seems to have been no increase. Whether these be the first indication of a coming epidemic remains to be seen.

I have very few notices of scarlatina in other countries. In Nova Scotia it attracted attention in August, 1871, and seems to have continued into July of the present year, most likely then having lost its epidemic force as in this country. In Berlin, in 1871, the mortality from scarlatina seems to have been 2.6 in 10,000, and from a notice in the weekly returns of births, &c., for London, to 5th October, it appears that there were 16 deaths from scarlatina in Berlin in the preceding week. Scarlatina was reported at the Cape of Good Hope in May last, and it continued in August, the date of the last information I have. It was also stated to be at Penang in February.

Diphtheria has not been met with to any remarkable extent in England since the middle of 1871, up to which period its course was noticed in my last year's address. It has shown itself in nearly every county at one time or other, and has occasionally presented considerable activity, as at Norwich during the fourth quarter of 1871, when the deaths were in the annual ratio of 3.5 in 10,000. When the whole year is taken, however, the mortality is materially reduced. In the eighteen English towns, the highest was 1.9 in 10,000 in Birmingham; the next, 1.5 in Liverpool, then 1.25 in Norwich, and 1.2 in Bristol, Portsmouth, and Sunderland. As with other epidemics, the exacerbations did not occur in all these places in the same quarter, and there was no approach to a definite proportion between the deaths from scarlatina and those from diphtheria, anywhere, or at any time.

In Scotland, the mortality from diphtheria in the Eight large Towns was 1.9 in 10,000 in 1869; in 1870 it was 2.7; and in 1871 it rose to 3.2. Taking the last quarter of 1871 by itself, the mortality was at the annual rate of 3.5 per 10,000, and it was even 3.6 for the first quarter of 1872;

but this was chiefly caused by an unusual number of deaths in January. From February onwards to August the annual ratio of mortality was 2.3 only, but in September the deaths were doubled again. As in England the deaths from diphtheria were distributed very irregularly among the different towns, and bore no definite ratio to those caused by scarlatina at the same time or place. In 1871, the highest mortality from diphtheria was in Edinburgh, when the ratio on the whole year was 7.6 in 10,000, while at Leith it was 4.3, and at Glasgow and Greenock, it was 2.4 only. The increase in September, 1872, was to a great extent in Dundee, which, in 1871, had suffered less than any of the other towns except Perth. There were 13 deaths from diphtheria in Dundee in September, and this without a single death from scarlatina.

In Ireland diphtheria does not seem to have been active in 1871 or '72. The mortality from this disease in Dublin in 1871 was only .7 in 10,000, and in the three quarters of 1872, which have now passed, the annual ratio seems to have been much the same.

In Berlin, in 1869, diphtheria carried off 873 persons, and in 1870, 555. In 1871, the deaths from diphtheria and croup together there, were 873 or 10.1 in 10,000 living, but the relative numbers I am unable to afford. In November and December, 1870, diphtheria was not infrequent at Malta, it continued through 1871, and seems only to have ceased to attract attention about March this year. During last spring and summer this disease was very prevalent and severe at Posilippo, near Naples. Early in the year it was also at Rome. At the Cape of Good Hope it was frequent in August last. In this colony, as well as in Victoria, its visits have been frequent, and severe, for a considerable number of years past.

The mean mortality from hooping cough in England and Wales for the twenty years, 1850-69, was 5.2 in 10,000. In 1869 and 1870 the deaths from this disease differed from the mean for twenty years by a minute fraction only. In the first half of 1871, the deaths for the whole country were only two-thirds of those for the corresponding part of 1870. In the third quarter, however, indications of an increase were visible in the returns for the 18 large towns, some of which presented somewhat higher rates than the preceding one. In the fourth quarter; twelve of these towns presented an increase, in many of them to a very considerable extent, over the third quarter, and their numbers were again higher

in as many instances in the first quarter of the present year, showing a gradual increase in the epidemic from about the middle of 1871 to the first quarter of 1872, subsequent to which it seems to have declined. The disease attained its maximum in Portsmouth, and Wolverhampton, in the fourth quarter of 1871; in London, Norwich, Bristol, Birmingham, Liverpool, Manchester, Salford, Bradford, Leeds, Hull, and Newcastle, in the first quarter of 1872, and in Sunderland in the following one.

In Scotland the average mortality from whooping cough, for the whole country, for the fourteen years from 1856 to 1868 inclusive, was 5.1 in 10,000, agreeing very closely with that for England. In the eight principal towns in 1869, the mortality was 14 in 10,000; in 1870, 10.4; and in 1871 it was 8.6. The disease diminished greatly from the first to the third quarter, but increased again in the fourth, as in England, the deaths being 273, 202, 149, 287 in each, respectively. In the first quarter of 1872, the deaths were 549, in the second, 506, subsequent to which they began to diminish, and in the month of September there were but 87. During the first two quarters of 1872, when the mortality was at its highest, 773 deaths, out of the total of 1,055, occurred in Glasgow alone, giving an annual ratio of 33 in 10,000. At Paisley, but seven miles from it, the ratio was 19; at Greenock, 17; and at Edinburgh, 12.4 per 10,000. In Ireland the epidemic was also experienced, but not so severely as in Scotland. I have not the means for showing when it first became appreciable there, but it seemed in full operation in Dublin, Belfast, Cork, Limerick, and Londonderry, in February, and, with the exception of Belfast, it diminished about July. Taking the deaths from February, to 13th October, the annual ratios of mortality in 10,000 living come out, in Londonderry, 13.7; Belfast, 6, Cork, 5; Limerick, 5.3; Dublin, 1.1.

From abroad I have notices of whooping cough being experienced at Shanghai and Formosa to a limited extent in 1871, and at Gibraltar from October to December that year. It was prevalent at the Cape of Good Hope in January 1872, and at Mauritius in March. It was at Corosal in British Honduras, from January to May that year, and in June it was prevalent in Barbadoes and Trinidad in the West Indies, and the same month in Nova Scotia.

In the remarks laid before the Society at the opening meeting of last session, I sketched generally the course of fever in Europe since 1868, observing that it had been pre-

sented to us under the forms of yellow, relapsing, typhus, and remittent, and that there was frequently such a connection between these in regard to time and locality, that one could not be separated from the others without breaking up the relation they bore to each other as constituent parts of the same general epidemic. It is clear, that if we desire to enlarge our knowledge of the laws of epidemics, we must examine their manifestations over a space not less than that they naturally occupy, and for a sufficient length of time, to embrace their development and decline, and every relation this investigation may establish, requires to be carefully noted, though its significance at the moment may not be apparent, or even if it should appear to militate against some generally accepted view. In the latter case, I need scarcely say, that should these relations be fairly established, the views they are at variance with will require modification so far as to include them.

In the third and fourth quarters of 1870, there was a considerable increase in the mortality from fever in Nottingham, Liverpool, Salford, Bradford, Leeds, and Sheffield, and at Newcastle in the fourth quarter. In the case of Liverpool, the disease continued frequent during the first and second quarters of 1871; at Bradford and Newcastle, the mortality remained above the mean of 1851-60, during the first quarter only, while Nottingham, Salford, Leeds, and Sheffield, it fell below it. During 1871, fever generally in England remained low, but at Nottingham, Leeds, and Sunderland, it again manifested considerable activity during the third and fourth quarters, while Nottingham continued a high rate into the first quarter of 1872, and Sunderland into the second: the mortality in the remainder of the eighteen large towns was low during the first two quarters; and with the exception of Portsmouth, Nottingham, Leeds, and Hull, which showed increased rates, all remained low during the third quarter.

Fever seems to prevail to a considerably greater extent in the principal Scotch towns than in those of England. Thus the mortality from it in the eight towns in 1869 amounted to 19.7 in 10,000, in 1870 to 14.5, and in 1871 to 13.8. During 1871, the ratio for Paisley was 23.9; for Greenock, 16.6; for Glasgow, 15.1; and for Dundee, 15.0. Edinburgh, Leith, Perth, and Aberdeen, were all much below the mean. The frequency of the disease diminished through the first and second quarters, was lowest during the third, and in the fourth again rose perceptibly. In the first quarter of 1872 the

deaths from fever remained much the same in number as in the previous one, and in the second, showed a reduction. In August and September, the actual numbers did not materially differ from those of the previous three months, but showed a smaller number of deaths from typhus, with an increase of those from enteric fever.

Fever has not existed to a very remarkable extent in the eight chief towns of Ireland during the present year; the Registration there is acknowledged to be very imperfect, but so far as the weekly returns show, those from February 3rd to October 12th, indicate the annual ratio of mortality from fever for Dublin to be 10 in 10,000 living; for Belfast, 9.8; Galway, 10.7; and for Londonderry and Waterford, 9 each; Cork, Limerick, and Sligo, are none of them half so great. There seems to have been little change of frequency in the deaths from the disease during the different months.

In England, since 1869, the deaths from fever have been returned under the three denominations of typhus, enteric, and simple continued, and in Ireland the same method was adopted at the same time. In Scotland, up to 1864, the deaths from this disease were placed under typhus, but, in 1865, a fresh classification was adopted, which has continued in force ever since. Under this, the deaths are distinguished as having arisen from typhus, enteric, relapsing, simple continued, and infantile remittent fever. Though no doubt the persons who return fevers under these denominations, are liable to errors in diagnosis, yet by keeping the various species before them, their determination will prove correct in the main, and there will be an average amount of error, which will not militate against useful conclusions being drawn from the numbers recorded. Thus we find that while the English returns afford no clue to the frequency or persistency of relapsing fever, though local epidemics of it may have been prevailing for months (as that at Tredegar, 1868-9), the Scotch show that cases had been met with every year from 1865. In the eight principal towns, the ratio of death from this fever amounted to 0.3 in 10,000 in 1869, to 1.5 in 1870, and 3.1 in 1871. The deaths in 1871 were most numerous, in the first quarter amounting to 138; in the second they were 63, in the third, 51; but in the fourth they rose again to 85; in the first quarter of 1872 they fell to 40, and in the second to 14, shewing the gradual disappearance of this form of fever. In 1871, relapsing fever was most frequent in Glasgow, when the annual ratio of deaths amounted to 5.2 in 10,000; in Paisley, where

the ratio was 4.0; and in Dundee, where it was 2.8. Greenock, though in the vicinity of Glasgow and Paisley, had a ratio of 1.8 per 10,000 only. In Edinburgh, Leith, Perth, and Aberdeen, the mortality from relapsing fever was much smaller, though it was met with in all.

There was a severe epidemic of typhus in Vienna in 1871, which seemed to terminate about the end of August. The admissions at the London Fever Hospital from typhus, which had been numerous in the last half of 1870, and first of 1871, then also had been much reduced. On the contrary, enteric fever, which had been comparatively infrequent during the latter part of 1870, and first half of 1871, increased materially from July onwards. In Dublin, Dr. Grimshaw noticed that the admissions at the Cork Street and Hardwicke Fever Hospitals, presented typhus in excess in 1870, and enteric fever in 1871. Corresponding with these changes, enteric fever seems to have become very prevalent in Paris, and several of the departments, in the latter part of 1871, and this apparently, in Paris at least, was still going on in April 1872. Fever has been remarkably prevalent in Rome for many months. This has been represented as malarial, and as enteric, probably a combination of the two, which is met with when the causes of both fevers are in operation at the same time and place. The mortality during September reached the annual ratio of 73 in 10,000 living. Enteric fever is usually considered to be endemic, and to be owing to causes in operation in the midst of the respective populations, facts such as those now given, suggest the operation of an epidemic factor at times, which may hereafter require investigation.

Early in 1871, during the pressure of the famine in Persia, a fever, said to resemble plague was reported to have broken out in Khorassan, the province to the south east of the Caspian, and subsequently it was mentioned that typhus and cholera existed together at Tehran, but I have not seen such a detailed account of these as would enable me to draw any practical inference from the statement. In the end of December 1870, cases of true plague manifested themselves in a mountainous district S.W. of Suleimaniyeh, close to the Turco Persian frontier; these went on through most of 1871. In December, an unusual amount of intermittent fever was met with at Gwadel, on the south coast of the Mekran, between the Persian Gulph and Kurrachee, and in the course of 1872, fever of a bad type was prevalent at Bushire, attended with great mortality. Fever also continued very prevalent

in the northern parts of Persia, supposed to resemble plague, so that a commission had been sent from Constantinople to examine into its nature. From the copy of a dispatch from the Ottoman Consul at Tiflis, dated August 24th, which had been communicated by the Turkish minister to the Board of Health at Constantinople, for which I am indebted to Dr. Dickson of that place, it appears that up to that date the commission had not reported. The consul adds, that though the heat during the summer had been very great, there had been no cholera through the whole extent of the Caucasus, but, on the contrary, there had been numerous inflammatory diseases, and that intermittent fevers had caused a mortality which was nearly twice as great as in ordinary years. Dr. Dickson, in his note, mentions that on 5th October, the Board of Health had received a telegram from Tiflis, stating that some cases of cholera had appeared on the 30th Sept. at Suram, in the Government of Kutais.

In the end of the past, and during the present year, dengue has spread very widely. It will be remembered that the first heard of this disease was from Zanzibar, where, in July 1870, after the cholera had ceased, dengue made its appearance and became very prevalent. Whether it affected the neighbouring country I have no positive information. The disease was next met with at Aden, where, in the end of June 1871, it broke out and prevailed extensively. Continuing on the same line, it seems to have been very frequent among the pilgrims at Mecca in April 1872, and cases were met with by a medical officer of the French navy at Port Said, at the Mediterranean end of the Suez Canal, some months earlier. In December 1871, it was very severe at Calcutta, and remained there for many months, not having quite abated until August 1872; during this period we hear of it at Cannanore in March, at Poonah and Sholapore in April, at Madras and at Deesa in June, and at Allahabad and over a wide district in August, at Trichinopoly in October, and in Burmah. These notices, culled from the ordinary sources of information, give an idea of the extent over which the disease prevailed, but until we receive the detailed official reports from India, it will be impossible to form a correct idea of the exact distribution of the epidemic, or of the dates of its prevalence, and its relations to the cholera epidemic especially, which has been met with in some parts of India this autumn. The dengue seems to

have shown itself at Shanghai about June, in addition to the numerous places it affected in India.

Dr. Christie, in his account of dengue, laid before the Society last year, mentioned that about forty-eight years before, that is in 1822 or 1823, a similar epidemic had been experienced at Zanzibar. We next hear of the same disease having been met with among the troops at Rangoon, in 1824 and 1825, at that time engaged in the first Burmese war. At the end of the year, and during 1825, dengue showed itself at Calcutta, and extended along the valley of the Ganges, pursuing a course, in fact, presenting much resemblance to that followed by the present epidemic.

Previous to the appearance of dengue at Calcutta, there had been a vast amount of fever in the Burdwan district in its neighbourhood; the mortality was immense, and whole villages were depopulated. In the early months of 1872 its ravages diminished, but even then the attacks were very numerous. No less than 114,042 persons having sought relief at the various dispensaries in the district during the month of April.

At Mauritius, the fever which had been prevalent in that island for so many years continued during 1871-2 to cause much sickness.

In the western hemisphere there was a slight epidemic of yellow fever at Monte Video in March, April, and May last, and this disease was more frequent at Bahia, Pernambuco, Para, and Maceico, on the northern coast of Brazil, about May, and from a notice by the Governor of Louisiana, published by the Board of Trade in the *London Gazette*, it would appear that the same disease is believed to exist pretty generally through the West India Islands, from Cuba and Jamaica to Trinidad, and along the northern coast of South America, and the shores of the Gulph of Mexico, but, so far as I have learned, it nowhere presents any great activity.

At New York there were dropping cases of cerebro-spinal fever in the end of 1871, and early part of 1872; from 13th January to 7th March, 31 deaths had been returned from this affection; from that date they increased rapidly, so that in the weeks ending 11th and 18th May they were 50 and 51 respectively; from this time the numbers fell, but in the week ending 20th July, there were still 21. At Montreal, cases of this disease had been frequent in the course of the summer.

Last year I gave a general view of the course of cholera

in Europe up to the autumn, and during the session we had papers from Mr. Radcliffe and Mr. Mitchell, on its course within the last two or three years in Russia and the neighbouring countries, which, from the sources of information they have access to, are very valuable. On one point, Mr. Radcliffe's information seems to have been inexact, and as it is an important one, I have no doubt he will pardon my allusion to it here. He was under the impression, that the outbreak at Broussa, on the sea of Marmora, in 1871, commenced there in August, and spread to the surrounding districts, whereas, from a detailed statement in the *Gazette Médicale d'Orient* of Constantinople, for January 1872, it appears that the disease was first seen at a village named Tchardy (Harmandjik on Keippert's map), 36 hours distant from, and south of Broussa, and with Mount Olympus intervening. From Tchardy, the disease spread to Taouchanly, on the road to Koutahia on the one hand, and to Kassaba and Broussa on the other. It prevailed at Taouchanly from July 8th to September 4th, and caused from 250 to 300 deaths; at Broussa it commenced on August 4th, and terminated on September 12th, having caused 51 deaths; at Tchardy there were 21 deaths. There were fourteen villages in this space, in addition to the above, in which there were 145 deaths. There was little intermittent or other fever previous to, or during the prevalence of the cholera, but, on its cessation, a violent epidemic of intermittent ensued. Importation could not be traced in this case according to the reporter, Dr. Mordtmann.

The wave of cholera which had embraced St. Petersburg in 1870, and, which, in 1871, was experienced from Altona on the North Sea, through northern Germany, and Russia, to the Jenisei in Siberia, and northwards to the Arctic Ocean, was passing away towards the end of that year.* It was succeeded by another, which was indicated by the occurrences in Persia in 1870, and which, by the autumn of 1871, had affected the district on either side of the Caucasus from the Caspian to the Black Sea, Astrachan, Taganrog, Rostov, and Kherson, and had spread into the provinces of Poltava and Kostroma, in a very active form,

* It was under this wave that the outbreak occurred in the steamer *Franklyn*, from Stettin to New York, in October 1871, detailed in the *Lancet* of February 17th, 1872. Two men who communicated with the *Franklyn* at Halifax were subsequently attacked, and some others. It is of importance to state, however, that in August diarrhœa, with pains in stomach and sickness, was not unfrequent among the inhabitants of Halifax, and a soldier's child had an attack of cholera that month.

and reappeared in Nicolaief, Ekaterinoslav, and Kiew. The attack at Broussa mentioned above, as well as the outbreak at Constantinople in September, and those at Sulina, Tultcha, and Galatz, on the Danube, in October and November, are all referable to this wave, as well as the outbreaks in Arabia and east coast of the Red Sea, up to the end of November. In 1872, the epidemic pursued its usual course to the northward, and appeared at Wilna in June, in St. Petersburg on the 11th of that month, and its activity was increased at many points to the southward, where it had been seen in the end of the previous year. Up to the end of July 1872, it had prevailed at Kiew severely, and more or less, in the Governments of Kherson, Ekaterinoslav, Kharkof, Voronez, Volhynia, Tchernigow, Koursk, Mohilew, Orel, Riazan, Moscow, and St. Petersburgh. In the Government of Ekaterinoslav not more than 800 deaths were recorded; in that of Kiew upwards of 3,000. While pursuing this course it crossed the Russian frontier to Insterburg, near Konigsberg, in July, to Warsaw and other places in Poland in August; to Roumania in September, to Gefle in Sweden in October; and it was at Helsingfors in Finland in August. As usual, too, cases undistinguishable from malignant cholera were met with a long way beyond the immediate range of the epidemic, though at places within the limits of the same pandemic wave; thus at Paris a case proved fatal about 25th February, which was described by Professor Bouillaud as malignant cholera, and in Berlin, on 29th July, there were three such cases also fatal.

Cholera is still active in Lemberg and Warsaw and their vicinity, and in the end of October was reported as having broken out at Buda. Within the last few days we hear of a death from it at Vienna, and another at Prague, and five at Dantzic, supposed to be Polish watermen employed on the Vistula.

In a very interesting communication to the last number of the *Lancet*, Mr. Mitchell gives the details of an outbreak of cholera in Central Asia. This appeared in Turkestan in the middle of April, at Tashkand, about 100 miles to the south of Turkestan, on 30th June, and at Khohan, 100 miles south-east, and Samarcand, more than 200 south-west of Tashkand, about the same time. We hear of cholera in Bokhara, in the same latitude as Samarcand, about this time, and in Cashmere, some 500 miles to the south, it appears to have become epidemic at least about June. In the opposite

direction it broke out along the Amou Daria, affecting the Russian forts on that river one after another, and ultimately reached the north east of the Caspian in the middle of August. The northern part of this outbreak clearly belongs to the same general wave just described, but before the Cashmere epidemic can be decided on more precise information as to dates is required.

There has been a very severe outbreak of cholera in India this year, 1872. This seems to have commenced in the Gangam district, on the east coast, in March, to have extended to that of Vizigapatam to the south in May, and to the Godavery district in July, with much mortality in the two former. Isolated outbreaks also occurred in the Central Provinces and Deccan. About the beginning of August, it also appeared in greater intensity in the Punjaub and North west Provinces, and involved most of the hill stations, as well as those in the plains. Cholera also appeared at Lucknow and Fyzabad in October, and a sharp outbreak among the sailors at Calcutta was reported by telegraph on the 12th of that month. This disease was also reported from the western Mekran, towards the entrance of the Persian Gulph in December 1871, and on the Red Sea, where the disease had ceased everywhere south of Yembo prior to the middle of November, except at Mecca (where cases continued till just before the period of the great pilgrimage); a fresh outbreak of considerable severity took place at Gonfodeh, 400 miles further south in December.

Some time in January 1872, apparently, or as it is described in the *Lancet* (October 12th, p. 531), just before the period of the great pilgrimage, the disease had ceased at Mecca and Gonfodeh, but prevailed at Hali, to the south of it. In January also, a caravan of 2,000 persons lost 89 from cholera between Medina and Mecca. The days of pilgrimage this year were from the 19th to the 23rd February, and though the number assembled was estimated at 80,000, there was no cholera at Mecca during the festival, though on the 27th February, when the greater number had probably departed, it made its appearance in the city, and there were 39 deaths up to 3rd March. On 10th March, cholera broke out at Hodeida, near the entrance of the Red Sea, on the 25th it appeared at Yembo, and about the same time at Medina. Medina is said to have lost 1,800, out of a population of 20,000, between the 20th and 28th of March, and the Damascus caravan, numbering 4,000, lost 400 from cholera. The disease ceased at Medina on the 3rd April,

and during this month it disappeared also from Yembo, Mecca, and the rest of the Hedjaz, and from the infected towns in Yemen. On the west of the Red Sea, however, in September, cholera broke out in the districts of Berber and Taka, in the latitude of Gonfodeh, and slightly south of Mecca. All these developments of cholera come under the same pandemic wave, which immediately follows that mentioned above as passing over Europe.

Within the last few days, a report on the climate and vital statistics of Tasmania, by Dr. Swarbeck Hall, one of our corresponding members, was placed in my hands by Dr. Milroy. This contains a detailed return of the causes of death in Tasmania each year, from 1868 to 1871, and another, giving the same facts for the Hobart Town Registration District, with the deaths arranged by months, for 1870. From these, it appears that in 1868 there were two deaths from cholera in Tasmania, and in 1870 they amounted to five, of which, two occurred in the Hobart Town district in January and February. These cases are of much interest, as having occurred at the most southerly point at which there has been any record of cholera, and, further, by their bearing on the question of pandemic waves. By the map on the wall, you will see that the isoclinal 70° S. passes through the southern part of Tasmania, and by the explanation attached, the advancing edge of the cholerific wave will be there at the commencement of a year with an even number, consequently we might expect cholera in years with even numbers to a greater extent than in those with odd. The facts mentioned agree with this, but their connection may be traced mnch farther. A wave proceeding from the isoclinal 70° S. in 1868, would reach that of 30° N. by the beginning of 1872, and we have this slight manifestation of a cholerific influence in Tasmania in 1868, continued by the epidemic at Zanzibar in the end of 1869, and beginning of 1870, and by those in India this year, each step corresponding with those indicated on the map before you.

In my last year's remarks, it was mentioned that in the early months of 1871, cholera was prevalent on the east coast of Madagascar and at the mouth of the Zambesi to the south of Mosambique, and sporadic cases were met with at the same time in Mauritius, and it was said, these corresponded, both as to time and locality, with a fresh wave from the southward. This wave would have advanced from the isoclinal 70° S. in the beginning of 1870, and Dr. Hall's returns now supply the fact that cholera was actually met with to some extent in Tasmania and Hobart Town that year.

Dr. Christie stated that dengue did not appear at Zanzibar in 1870 until cholera had ceased, and the same seems to have taken place at Mecca in April last, if our information is to be relied on. At Calcutta, on the other hand, cholera does not seem to have commenced until dengue had almost disappeared. The comparatively narrow portion of country invaded by the cholera epidemic in Upper India this autumn, suggests there may have been some such cause of limitation there, and that dengue may in this respect, stand in the same relation to cholera as other fevers, but until we receive more detailed information as to the distribution, and time of prevalence, of these diseases, respectively, throughout that country, this question must remain in abeyance.

The details given above, afford some illustration of the inability of fever and cholera to exist together as epidemics at the same place. Thus, from Persia, with febrile disease so generally diffused through it in 1872, we hear nothing of cholera. At Broussa, in 1871, immediately after the disappearance of cholera, a general and severe epidemic of intermittent fever ensued all over the district. And in the country south of the Caucasus, with an unusual prevalence of periodic fever during the summer, nothing was heard of cholera until the end of September, when a few cases occurred at Suram.

I had intended to have offered some remarks on the recent extension of some of the diseases of cattle, etc., but the length to which these observations has already reached, precludes my doing so on this occasion.

MEDICAL STATISTICS, WITH ESPECIAL REFERENCE TO CHOLERA AND SYPHILIS.

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[*Read December* 11*th*, 1872.]

There is, probably, no branch of human knowledge to which the application of the numerical method is better fitted, and from which it is calculated to yield a richer harvest of valuable truth, than to the logical interpretation of the facts of medical science.

There is, at the same time, certainly none other in which it is more necessary to guard against the fallacies incidental to, and, in some degree inseparable from, this method of inquiry; in which more rigorous accuracy is necessary in the collection of the facts themselves, and in which greater caution is demanded in avoiding hasty or dogmatic deductions from mere numerical results.

If it be true that the medical art is entirely one of observation, it is equally true that these observations should, from time to time, be subjected to the strict scrutiny which mathematical science alone is capable of supplying. By this means only can we hope to eliminate from them the sources of error to which all human observations are liable, and to deduce from them the general laws by which the phenomena to which they relate are explained, and from which the practical experience is to be derived that, in the eloquent language of Andral, "formulates the past, renders the present fruitful, and prepares the future."

At the same time it cannot be denied that the facts of medical science are so complex in their nature, so liable to be influenced by an infinite number of collateral and minor considerations, which it is well-nigh impossible to take into account with the minute detail necessary to absolute accuracy,

as to render the application of numbers to their solution, much more difficult than to the interpretation of the facts of physical science, or even to those of social economy, which form the only sound basis of modern legislation.

The careful collection of the statistics of population in the admirable returns of the Registrar-General of England, together with the periodical census now taken in all civilised countries, leave little to desire in making us acquainted with the logic of the facts to which they refer.

But there is a logic underlying the facts which they do not and probably are not intended to explain. They illustrate the cardinal conditions in which we live and die, increase and multiply, and toil and moil in the great struggle imposed upon us by modern society and civilisation. They likewise show us the weak points in our armour of defence against the thousand agencies constantly at work to deteriorate or destroy the fair fabric which has been created as God's own image, and to abridge the natural span of existence accorded to it by the Divine Creator.

The very valuable records of the Health Department of the Privy Council, those of the Local Government Board, and the numerous sanitary reports perpetually issuing in never-ending streams at home and abroad, deal more or less completely with the weak points above referred to, endeavour to ascertain their causes, and to suggest remedies for their removal.

But the very abundance of the materials at our command is itself a cause of confusion in dealing with the facts themselves, and so long as they are not subjected to rigid analysis, in the only manner by which we can trace back the effect or incidence of particular diseases to their exact cause or causes, so long shall we be unable to deal with them efficiently, in the measures necessary for their mitigation or removal.

It would be out of place at the present time to consider or discuss the principles which should regulate the collection of medical statistics; yet it would not be without interest to ascertain why those principles are so little understood or applied now-a-days, when we are probably in a better position to use them with effect than were our predecessors at any period since the discovery of printing, or since the more exact methods of inquiry and observation, now in general use, have been taught in our schools.

Be that as it may, I think that the International Statistical Congress which met during the past autumn at St.

Petersburgh is entitled to some credit, for having included in its programme the statistical investigation of two such important diseases as cholera and syphilis.

The purpose of my presence here this evening is to make you acquainted with what was said and done there, for it is manifest that such questions as those mentioned are not of local or temporary interest, and that to arrive at sound conclusions regarding them is of as much importance to us and to all other civilised nations, as to Russia and the different nationalities comprised in that great empire.

Before proceeding, however, to the immediate subject of my paper, I crave your indulgence and permission to say a few words regarding the action of the international representatives of statistical science, in relation to questions of life and health, from its origin to the present time.

These questions have been arranged under two heads viz., population and hygiène. Of the former, the only particular strictly relating to vital statistics, has been that of mortality tables. Census returns and all matters relating to movements of population are rather questions of political and social economy, than of biology properly so called and considered. The framing of mortality tables, again, has not been regarded from its biological side, but in relation to insurance and survivorship.

In relation to hygiène or public health, the questions considered prior to the present year had reference to geographical influences on health, statistics of deaths, of epidemics, of insanity, of sanatory establishments, military medical statistics, comparative statistics of health and mortality in civil and military populations, the statistics of accidents on public ways (railways, common roads, rivers, and canals), in great public works, in mines, and in factories.

The subject of the statistics of epidemics was discussed at Paris in 1855, and excellent rules were framed for the general conduct of such inquiries. In the resolution of the Congress it was stated that the programme adopted could be adapted in extent and detail to the locality invaded, but the necessity was emphatically enjoined of embracing in the inquiry the greatest number of questions possible. It was also insisted that the particulars most difficult to gather, instead of being the least useful, are probably the most important for the solution of the numerous economic, hygienic, and purely medical questions included in or raised by the study of epidemic visitations of disease.

In the epidemics which have occurred since 1856, it is to

be regretted that some general international system of record, similar to that sketched by the Paris Congress, has not been acted upon, so as to render the experience of one nation useful to all. In these days of free and rapid intercommunication, when the approach of the storm or the epidemic can be communicated to distant places in ample time to make provision against their invasion or advent, an outbreak of pestilence or a disturbance of the elements in the remotest quarters of the globe, is of more or less immediate interest to the whole world. To us in England it is of probably more pressing importance than to most other nations; for while our insular, and so far isolated position, gives us some advantages in keeping out enemies of all kinds, our locomotive habits and our intimate commercial relations with all inhabited countries, renders us particularly liable to the importation of such morbific agencies as can be carried by human beings, or conveyed in a thousand ways, in the multitude of things which find their way to our markets and to our homes.

Our national habits, and the reliance on individual exertion, to which we owe so much of our advanced position amongst nations, is probably some, if not a great hindrance to the conduct of scientific enquiry in such manner as to utilise the observations and experience of the many, by the skilled labour of a few. It is only by a central authority, armed with the power of compelling the collection of the facts in a proper manner, and possessing the knowledge and skill necessary to utilise them, that this desirable result can be obtained. Hence, I sometimes hear with regret, expressions indicative of what I cannot but consider a misapprehension of the real uses of a central authority.

If rightly and judiciously directed, I regard such central authority as a fly-wheel to equalise and regulate the action of local self-governing bodies, and in no way to perform their functions or to interfere with their full and legitimate liberty of action within the limits of the laws by which we are all bound.

Hence, also, I believe that the sanitary legislation of 1872 —which does not profess to be a complete or final work—is emphatically a move in the right direction. It gives to all places local sanitary authorities, as good perhaps as can be expected in existing circumstances, when public opinion is so imperfectly educated in regard to sanitary matters. In a country like our own it is of no use to legislate too far in advance of public opinion. The same legislation provides

a central ruling power to regulate and harmonise local action, and to assist and advise local authorities in the numerous difficulties incidental to all sanitary inquiry and action.

I hope, hereafter, when another, but a far more difficult step in advance is taken, that many existing difficulties in dealing with questions of public health, will either disappear or become easy of solution. I allude to the consolidation of our scattered, timid, tentative, and in some cases obscure and obstructive sanitary legislation, with some plain, intelligible, comprehensive Act, free from the cardinal defects of much of our modern legislation, viz.—the vain effort to comprise in an Act of Parliament, by definitions and cumbrous details, the full and effective working of any law affecting such complex and varied interests, and such ever-changing circumstances, as are associated with the question of public health.

When this shall have been done, it will not be difficult to obtain such detailed particulars as will extract from each epidemic visitation or endemic incidence of disease, the information necessary to control future outbreaks, or, if possible, to prevent their recurrence by stamping them out as effectually as the plague has been stamped from our midst, or as small-pox might, and syphilis ought to be, if proper means were enforced for their extermination. Unpopular as the doctrine is commonly, and opposed as it is I believe erroneously, considered to be to the personal liberty which is the birth-right of every subject of the British crown, I hold that in matters affecting the public health, and, in consequence, the life, happiness and well-being of the community at large, all legislation should be compulsory, and no one should be permitted, from any selfish motive, to spread abroad pestilence, any more than he should be allowed with impunity to set fire to his own house, without heed of the risk of injury and destruction thereby caused to the dwellings of his neighbours.

The only special subject considered in its statistical relations by the International Congress, is the important one of mental alienation. This, also, was discussed in Paris in 1855, and it would be difficult to over-estimate its importance to all modern civilised communities. Indeed, it would seem, in some of its more important bearings, to be one of the penalties of civilisation itself; and as the pace at which we live grows faster, the struggle for life becomes fiercer, men congregate more closely in central commu-

nities, and the powers of life are, in consequence, more severely taxed, so do aberrations of reason appear to increase, and to demand the most careful attention of the physician, the moralist, and the statesman.

The further consideration of this topic, however tempting, is not in my programme, but one additional observation is suggested by it which is strictly relevant to the matter I have in hand. No systematic attempt has yet been made to give effect to the deliberations of the International Statistical Congress in this, or in any other of the important hygienic questions on which it has expended much time and thought. Equally little are medical statistics cultivated, in their strictly scientific relations.

I have been at some pains to endeavour to ascertain the cause of this, and it seems to me to be partly due to a somewhat general defect in the preliminary education of medical men, and in part to a too great reliance on observation and experiment as the only sound means of advancing medical knowledge. The neglect of the higher mathematics, and the comparatively imperfect standard of general knowledge which candidates for our degrees and diplomas are, as a rule, required to bring to the study of medicine, is, I think, much to be regretted in the interests of that noble calling both as a science and as an art. Our periodical medical literature, rich as it is in varied and interesting observations, exhibits much painful evidence of the absence of the close and logical reasoning, and of the rigorous exactness without which most observations lose their chief value as permanent contributions to our stock of knowledge. Our sanitary and other published reports contain a vast and bewildering array of figures, professing to represent facts. But, with a few illustrious exceptions, no one seems to be at the trouble of examining these figures and analysing these facts, in the only way in which they can be of any real use for practical purposes. It is piling Pelion upon Ossa for no better end than to accumulate an unwieldy and impracticable mass of materials to bewilder statesmen, and strengthen sceptics of the soundness of medical science, in their unbelief. The Congress is itself not without blame in the matter, as I shall show anon, when speaking of its recent action in relation to cholera and syphilis, to the consideration of which I shall now address myself.

To Russia both the questions are of immense import, and they are of scarcely less consequence to the rest of the world.

The Russian Empire now abuts upon our possessions

in India, is more immediately affected by outbreaks of cholera in that distant quarter than any other country, and is, therefore, more directly interested in endeavouring to ascertain the cause of the pestilence, with a view to its prevention. To the western world, Russia is the gate through which cholera is most likely to find its way, to desolate their hearths and homes, and in Russia itself the disease seems to be taking a permanent stand, and to be localised. The thoughtful, prescient, and logical letter of Mr. Radcliffe, on the recent diffusion of cholera in Europe, shows how much the former danger is increased, and how pressing it is. A valuable volume recently published by Dr. Hubner of St. Petersburgh, exhibits the statistics of all diseases in that wonderful capital in 1870, and among them cholera.

From a separate note kindly furnished to me by Dr. Hübner, and which he promises to expand for the *Epidemiological Society*, it would appear that correct records of cholera in St. Petersburgh have only been kept since the taking of the census in December 1869. In 1870 and 1871, there were three distinct epidemic visitations of cholera in the capital of Russia. The first began in August 1870, and lasted until March of the succeeding year. It caused 980 deaths. The second visitation lasted three months, March, April, and May, of 1871, and caused 1,310 deaths. The third commenced in June 1871, and caused 1,910 casualties. The death rates from cholera to general population in the two years, was 6.27 per 1000, in the two years.

In 1872, there was another visitation, which was gradually dying out when I left St. Petersburgh in September last. Of this, I have no detailed particulars, but I am afraid that cholera must now be regarded as endemic in the great northern capital, and that to justify a diligent and patient inquiry regarding it, there is no need to look further south for evidences of former epidemics, or for fresh importations of cholera material from more distant sources.

What then are my grounds for believing that this wandering and migrating pestilence has abandoned its nomadic character in Europe, and found a resting place under the window, through which Peter the Great intended his empire to look upon Europe.

It is based upon the extraordinary insanitary condition of St. Petersburgh, of which the sub-soil is so saturated with sewage matters, that it would scarcely be a mis-statement to say that the capital floated upon a bed of sewage.

The majority of the houses in that city have porous and

permeable cess-pits, into which all excreta find their way, and which are emptied three or four times a year, causing noxious exhalations during the process. The fine broad canals which intersect the city, are great open sewers. The amount of deposit in them is very great, and the authorities are said to be afraid to empty them, lest they should cause a pestilence. There is no pretence of drainage in the city, and I was informed that the water used by the poor for domestic purposes is of the most impure description—in fact, taken from the sewage canals above mentioned. What better and more suitable abode could be found for King Cholera, than this excrement tainted soil, excrement tainted atmosphere, and excrement tainted water-supply.

The Russian authorities, are, I believe, fully alive to the existence of these evils, and it is to be regretted that they do not deal more energetically with them, both in their own interests, and in those of neighbouring nations.

Regarding the great leading facts connected with cholera when it has broken out, we are all pretty well agreed, although we differ somewhat as to the importance to be attached to the exact manner of its extension, and some of us are disposed to view with unnecessary despondency the alleged helplessness of curative means, to save the lives of those attacked.

The cause of the pestilence is still, however, a sealed book, an impenetrable mystery, and it is to solve this that our earnest efforts should now be directed.

Preventive are always of greater value than curative measures in medicine. The latter can only reach individuals, and although medicine has no reason to be ashamed of the substantial advances made in therapeutics, greater reliance should undoubtedly be placed in the former, inasmuch as they may, and do affect whole communities.

What direction then should our inquiries take, to trace back the effects to their causes? I believe that it is in the scientific application of the numerical method, to the rigorous analysis of the facts and observations which are daily collected with remarkable patience and intelligence, in all countries to which cholera is an unwelcome visitor, that the solution will be found.

A careful and minute record of the exact conditions connected with each outbreak of cholera, and as far as possible, of each case, will, when analysed, show the circumstances or conditions which are accidental, and those which are constantly present.

Having thus cleared the field, it is surely neither visionary nor unreasonable to hope that the application of the many exact and delicate methods of examination now employed in scientific investigation, may so far reveal the immediate cause of the origin of the pestilence as to enable us to control, if it does not provide us with the means of extinguishing it.

It is necessary then to bear in mind that the statistical investigation of cholera will not itself solve the problem. It is simply the light to guide and keep us in the right path, and to prevent our straying into labyrinths of useless speculation and mystification.

It is the lens which will concentrate the rays of such knowledge as we possess or can gather to a focus, and, as in the case of light, cause it to burn with a brilliancy and intensity which it does not possess in its scattered and diffused state. Gavarret, in his tenth proposition of the general principles of medical statistics, has stated wisely and well: "Toutes les fois qu'il s'agit d'étiologie, les principes des lois des grandes nombres ne peuvent servir qu'à prouver l'existence ou le non-existence d'une cause spéciale soupçonnée indépendamment de toute hypothèse faite sur la nature. C'est à l'aide de considérations d'un autre ordre qu'on doit chercher à determiner la cause elle-même: cette dernière question est hors de la sphère d'activité de la statistique."

The other special question considered by the St. Petersburgh Congress was that of Syphilis, a question, unhappily, of almost equal interest and importance; for while it does not startle us by violent explosions and great immediate mortality, it is an insidious poison, ever in active, although somewhat secret circulation, and threatens a moral and physical degeneration of the human race, which can and ought to be prevented by every means in our power.

Unhappily, among ourselves, the investigation with a view to the prevention of syphilis has been made a subject of theological controversy and political agitation. While I respect and honour the motives which have led the few who are really earnest and honest in their opposition, to this demonstration of feeling, I cannot but consider them to be founded on misapprehension, and when calmly and fairly considered, to be entirely opposed to the undoubtedly pure and benevolent end intended by all honest advocates of legislative measures to regulate the public health in such matters. Those are, however, I am sorry to think, few in number.

The consideration of the questions of cholera and syphilis at St. Petersburg, was assigned to a sub-committee, which consisted chiefly of medical men, and was presided over by the academician, Dr. Middendorf. The secretaries of this sub-committee were Drs. Benezet and Bredow. From being a sub-section, the proceedings were not formally reported, which was, I think, much to be regretted. I attended two of the meetings, and was much impressed with the sound knowledge of the subject, the calm and philosophic spirit, the entire absence of foregone conclusions, and the earnest regard for truth which characterised the treatment of these important questions by our Russian brethren. Excellent charts of two epidemic visitations of cholera were submitted by Dr. John Erichsen, of St. Petersburg, who has kindly promised to send duplicates of them for the information of the Epidemiological Society, on the understanding that they are to be regarded as an incomplete work, intended to illustrate one of the means of advancing our knowledge of the subject, and of placing the leading facts regarding it on permanent record in an intelligible form.

I think it best to place before you the *ipsissima verba* of the St. Petersburg proceedings, both because they led to some difference of opinion in the general assembly of the congress, and because some of the recommendations, although not strictly applicable to countries having a complete sanitary organisation and a better provision of medical agency than are to be found in Russia, are, nevertheless, of great general interest.

Respecting cholera, the following resolutions were adopted by the sub-sections, and recommended to the Congress to be followed in the future collection of facts on the occurrence of cholera:—

General Directions.

1. When cholera invades a locality, there should be instituted in that locality one or more sanitary commissions to divide the field of observation, or to name a special functionary, who, beyond the regulation of sanitary measures, should be charged with the direction or supervision of the collection of statistical facts relating to the epidemic.

2. The facts or particulars required to be collected relate (*a*) to the individual attacked and to his manner of life; (*b*) to the locality invaded by the epidemic.

A.—*Particulars relating to the individual attacked and to his mode of living.*

3. The particulars under this head should be collected by means of individual bulletins.

4. These bulletins are printed on the outbreak of an epidemic, and sent blank to the sanitary authorities, hospitals, medical practitioners, householders, parish priests, and all the authorities of the place.

5. The individual bulletins should have a single heading entitled "Cholera," without distinction of the period of the disease, and should furnish the following particulars :—

(*a*) The quarter, street, number of the house, and of the lodgings, as well as the floor where the case has occurred, or the individual has been attacked by the disease.

(*b*) The name, surname, and such other particulars regarding the individual, as are required by the Census returns.

(*c*) The mode of living of the individual as regards his dietary, if he led an active or sedentary life, if his work entailed great mental or physical exertion, or if he exercised his mental and bodily powers with moderation, if he were temperate or the reverse, &c., the circumstances, in the opinions of those about him, which appeared to cause the attack (indigestion caused by any particular article of food, a chill, and how caused, mental, moral affections, &c.).

(*d*) The water drank by the patient (spring water, rain water, well water, &c.)

(*e*) Had he been in contact with other persons suffering from cholera, or had he been in their society shortly before his attack, at what exact time, or in an invaded locality.

(*f*) Does the sufferer exhibit any permanent or temporary pathological or physiological particularities (infirmities, hernia, pregnancy, &c.).

(*g*) Is the attack the first from which he has suffered; if not, how many times he has been attacked, and in what epidemics.

(*h*) The day (date and month) when the first symptoms of the disease manifested themselves; the first medical aid secured; the transport of the person attacked to a hospital, if he has been taken to one; the issue of the attack in death, cure, or secondary (consecutive) diseases.

(*i*) The particulars required by the Census returns regarding the lodging of the individual, with an indication of the number of occupants at the time of registration.

(*k*) The cases of cholera which may have occurred in the

house or lodging of the individual attacked, before or after his sickness, who were the individuals attacked, did they die, were they sent to hospital, or are they still under treatment, in progress of cure, towards a fatal result, or discharge from hospital.

(*l*) Was disinfection employed of the clothes or dejections of the sick man; if so, what means were used.

6. The first entries in the bulletins, are to be made on the earliest medical aid given to the person attacked. When he dies or is cured, the remaining details are to be filled in, and the report then sent to the proper authorities.

7. The bulletins are filled in—

(*a*) For sick treated in the hospitals by the medical staff of the institution, who interrogate the sick or the persons who bring them.

(*b*) For patients treated in their own homes by the persons surrounding them, with the aid of the medical attendants who have treated the case; and when those persons are unable to afford the desired information, by a delegate of the sanitary authorities, whom those persons are bound to inform of their inability to fill in the bulletins.

B.—*Particulars relating to the invaded locality.*

8. In large towns (containing more than 10,000 inhabitants) every house in which a case of cholera occurs receives a form (bulletin), on which the following particulars are to be recorded:—

(*a*) The quarter (parish), street, number of the house, and all particulars required by the Census, with an indication of the number and character (nature) of the population, as contained in the Census returns.

(*b*) The nature of the accommodation provided for natural wants (water-closets, privies, &c.), the manner in which they are kept, their cleanliness, and the means adopted to remove their contents, the state of cleanliness and general arrangement of all court yards, areas, &c.; the means of disinfection employed, if any, and how they were used.

(*c*) The existence of a well on the premises, and, in this case, the distance of the surface of the water from that of the ground, before and during the epidemic, if it be possible to procure such particulars.

(*d*) A nominal list of the persons attacked, and of those who died during the epidemic, with an indication of their apartments, of the time of their attack, and of the disappearance of the epidemic.

9. These bulletins are to be filled up by the sanitary authorities (commissioners) or by the divisional inspectors appointed by them. They are not to be closed until after the cessation of the epidemic in the quarter (locality).

10. In villages and in small towns (with less than 10,000 inhabitants) the household bulletins are replaced by a short description of the meteorology, medical topography, and climatology of the place, which should be as precise as possible.

11. These descriptions are to be made by the sanitary authorities of the place or by the persons specially charged to watch the progress of the epidemic, and should contain the following indications:—

(*a*) The name of the village, borough, or town, of the district and province in which it is situated, and its distance from the chief town of the canton or district (county town).

(*b*) The number of houses and inhabitants according to the latest Census returns.

(*c*) If the village, borough, or town be on the banks of a river (what is the river, is it embanked), a canal, a tank (pond), or on a high road, a causeway (by-road), a railroad, &c.

(*d*) If there are in the village, borough, or town, or in its vicinity, springs of water or wells, and, in the latter case, the distance of the surface of the water from that of the soil before and during the epidemic (if possible).

(*e*) If the adjacent or surrounding country is flat or hilly, dry or marshy, well wooded, if there is or is not a forest in the vicinity, or if the country is denuded of wood.

(*f*) The nature of the soil of the environs—whether black mould, argillaceous, sandy, rocky, calcareous, &c.

(*g*) Is the village, borough, or town situated in a plain, on the top, the declivity, or at the base of a hill, at the bottom of a large or small valley; is the soil at the level of the river, lake, or pond, or is it above their level?

(*h*) Are the houses built in straight or irregular streets, in large streets, or streets at right angles, in a single street or range, or are they dispersed in separate groups; are the houses in contact with each other or separated from each other by large intervals, and have they gardens around them?

(*i*) Are the houses well or ill built—at the level of the soil, above or below it; of what materials are they constructed (wood, brick, stone, &c.); are they low in the roof and small?

(*j*) Are there any factories in the village, borough, or

town, or in its environs, their position in relation to the place, up or down stream; what are the factories?

(*k*) Are there in the environs any great bodies of men (camps, barracks, fairs, pilgrims, shrines, &c.); their position relative to the place; up or down stream?

(*l*) The month and day (date) of the outbreak of the epidemic; the dates of the deaths and their number from the beginning to the end of the outbreak; the month and date of the end of the epidemic.

(*m*) Was the first recorded case in an inhabitant who had not left the locality, or was the disease imported by an outsider? In the latter, whence came he, when did he arrive, was he sick on his advent, or did he sicken subsequently; did he come from an infected locality?

(*n*) Did the inhabitants receive medical aid, and, if so, from a medical practitioner of the locality or from a stranger to the place?

(*o*) Did the epidemic manifest itself in isolated cases in different houses, or had it many victims in the same house? In the latter case, did these houses differ from other houses in the locality as to its hygienic state?

A part of the village, burgh, or town has suffered particularly; in this case, what is the situation of that part—is it near to the water, a marsh, or a cemetery? Are the houses smaller, and is its population poorer than those of the rest of the locality?

12. The two questioning forms, the one for large and the other for small localities, shall be printed in the same bulletins, in large type for the greater and in small for the lesser, so that in the smaller localities and wherever it is possible, the information required in the more important questioning may be furnished as far as possible.

13. All bulletins relating to the statistics of cholera shall be forwarded to the central medical authority, if any exists, or the central statistical institution of the state, there to be examined and analysed with the aid of a medical man.

In addition to the above, specific proposals were made to secure an examination in all large towns of the sub-soil water in relation to the theory of Pettenkofer, to add a special biological and medical section to all future international statistical congresses, and to entrust the consideration and discussion of all biological and medical questions to this section, when such questions form part of the programme of the congress.

The reception of the deliberations of the sub-committee

honours lavished upon the heroes of war were bestowed upon the soldiers of peace, would not the result be an unmixed blessing such as no war, however righteous and successful, can ever hope to convey? Would it not, indeed, justify the eulogy of the Roman orator, that "*Homines ad Deos nulla re propius accedunt quam salutem hominibus dando.*"

The result of an animated discussion at St. Petersburg on the cholera proposals of the Sub-commission led to the submission of two resolutions, which were, to a certain extent, compromises.

That of M. Castiglioni, which was rejected, was to the effect that "The eighth session of the International Statistical Congress approves the fundamental conclusions of the report on the statistics of cholera and syphilis, and recommends the questionaries annexed to it, referring the latter to the permanent commission of the Congress, which shall subject it to a definitive examination from the point of view of technical statistics, while carefully bearing in mind the spirit pervading it from the medical and sanitary sides."

That of Dr. Cyon, which was adopted, ran thus:—

"Considering the limited number of foreign physicians present, the Congress, without entering into a profound discussion of the programme elaborated by the Sanitary Commission, expresses a hope—

"That the medical men and statists who are engaged in the registration of cholera and syphilis take, as much as possible, as guide the programme proposed; and that the Russian physicians, in particular, apply it to the investigation of the epidemics so frequent in Russia, and present to the next Congress, a report defining the extent to which their programme is susceptible of practical application."

Statistics of Syphilis.

As in the case of cholera, so in that of syphilis, I lay before you the exact text of the labours of the Congress. The subject has been so little considered in its statistical relations, as to render it desirable to present it to the professional public in England in this aspect.

"The grave importance of the effects caused by syphilis upon the physical and moral health of a population, as well as its influence on its beauty and reproductive powers, are so universally recognised as to render it superfluous to

prove the urgency which exists; both as to our knowledge of the disease and to the practical measures necessary to combat it, to express in figures the degree of development of the malady among a people. Unfortunately, the methods adopted in Russia and other countries to collect these statistical facts, have been followed with so little method and system, as to render it impossible to judge from figures alone, of the ravages produced by this calamity in its various forms."

The International Statistical Congress has treated the question of syphilis in the following relations:—

(*a*) The nomenclature accepted by the Congress held in Paris, mentions only constitutional syphilis in indicating the different degrees of the infection, and the nature and seat of the lesions caused by it.

(*b*) In the statistical returns of hospitals, first class, second order, they adopted the division, into gonorrhœa and primary syphilis (without explaining whether it comprised only syphilitic chancres or also soft or venereal chancres); and the further division, perfectly useless, of secondary and tertiary syphilis.

(*c*) At the Congress of Berlin it was decided that in the statistics of recruiting a certain classification of disease should be observed, among which was ranged syphilis.

(*d*) It appears to us that preceding Congresses have left an important lacuna (blank) in their statistical tables of the causes of mental aberration, in omitting syphilis, which is one of the most frequent of these causes.

"It appears to us, therefore, that rules should be framed to render the statistics of syphilis useful to science and to the wants of medical and sanitary practice. A summary of these rules is subjoined."

A.—*In relation to the form of the disease.*

1. To separate rigorously syphilitic infections from other diseases resulting from sexual relations (chancre, gonorrhœa).

2. To recognise as syphilitic affections of the genital organs (syphilitic chancre) only those affections which have produced (*engendré*) general infections.

3. To register separately cases of fresh infection and relapses (recurrences) of the disease; that is the only means of learning exactly the powers of propagation of the malady.

B.—*In relation to the means of transmission of syphilis.*

1. The cases of infection from sexual contact, the surest

data being obtained from the inspection of prostitutes and soldiers.

The facts collected in medical institutions where suspected persons are treated, would serve to control the figures collected, and that is why the data furnished by inspection need absolutely to be verified by those of hospitals.

2. The cases of infection in other ways—(*a*) by lactation from the nurse to the infant, or *vice versa*; (*b*) by vaccination; (*c*) by the common employment of instruments in factories (those of glass, for example); and (*d*) by the common employment of household utensils, linen, &c., in congregate life.

3. The cases by hereditary transmission.

Remark.—It would be in the highest degree interesting to study the degeneration of populations resulting from syphilis, which is transmitted from generation to generation in countries where intellectual cultivation is little developed or where endemic syphilis begins (syphiloides), *e.g.*, in the north of Siberia, in Norway, Dalmatia, in the islands of the Archipelago, in Turkey, &c.

C.—*In relation to the degeneration of the population.*

1. Cases of abortion and premature deliveries caused by syphilis—(*a*) of the mother, (*b*) of the father. Syphilitic degeneration of the placenta to be noted, if possible.

2. Cases of death a few days after birth from want of vitality of the new-born, the result of syphilis—(*a*) of the mother, (*b*) of the father.

D.—*In relation to the influence of the disease on the nervous system.*

1. Cases of mental aberration.
2. Paralytic affections.
3. Epilepsy and other nervous diseases.

E.—*In relation to its influence on deformity.*

1. Deformity of the face.
2. Deformities of other parts of the body (the fingers, toes, &c.).

F.—*In relation to fitness for military service.*

1. Cases of release from time on account of incapacities resulting from syphilis.
2. Cases of exclusion from the service on account of incapacities determined by syphilis.

The collection of the data on syphilis is obligatory in—

1. Hospitals, ambulances, lying-in hospitals, foundling hospitals, asylums for infants and nurses, houses of detention.

2. Among land and sea forces; these corps should be regularly inspected, as they are in Russia.

3. In sanitary bureaux and places of inspection of prostitutes.

4. It would in addition be desirable if medical officers attached to or inspectors of factories, manufactories, of large collections of men, or those who inspect great bodies of people would take means to collect facts bearing on this question.

"The detail in which the statistical data connected with syphilis can be gathered in any country, depends upon the proportion of medical men to the mass of the population in that country; but, it is to be regretted that even in countries well off in this respect, central points have not been fixed where these facts can be centralized. This circumstance is so prejudicial to the uniformity of the materials already collected, as to render it nearly impossible to work them into a harmonious whole. The natural centres for the collection of these data should be, in our opinion, the medical administrations (sanitary bureaux), if they have statistical sections, or central statistical offices. Unhappily, the medical administrations of all countries do not embrace the extended study of syphilis in their operations, but restrict their action to a somewhat defective study of syphilis propagated by prostitution."

The programme on syphilis of the sanitary sub-section of St. Petersburg was not discussed at all in the general Congress, in consequence of the presence of ladies. While I personally should be most sorry to banish the better half of society from gracing scientific associations with their presence, there are some subjects which should not be considered in their presence, and this is emphatically one of them. Hence I consider the action of the Congress in this particular to be deserving of approval and imitation.

Although banished from the general assembly, one important branch of the inquiry was carefully considered in the 2nd section, under the presidency of Dr. Farr, when the following resolutions were recorded:—

"1st. The Congress recognizes the great importance of a serious study of prostitution, and insists upon the necessity of establishing in all large towns a sanitary control,

which, at the same time, should take charge of the special registration of all persons addicted to prostitution.

"2nd. The Congress recommends for the primary inscription the annexed questionary, and for the compulsory register the following columns:—1. Date of inscription; 2. Category of the inscription; 3. Date of transcription from one category to another; 4. Date of radiation; 5. Cause of radiation."

I cannot better conclude my notice of this branch of the subject than in the eloquent and touching words of an able historian of European morals—Mr. Leckie—to show how necessary is the inquiry, and how great is the reproach which attaches to us as a nation professing a higher standard of morality than our neighbours, for our manner of treating it.

"There has arisen in society," says this great searcher after truth, "a figure which is certainly the most shameful, and, in some respects, the most awful, upon which the eye of the moralist can dwell. That unhappy being, whose very name is a shame to speak, who counterfeits with a cold heart the transports of affection and submits herself as the passive instrument of lust, who is scorned and insulted as the vilest of her sex, and doomed for the most part to disease and abject wretchedness and an early death, appears in every age as the perpetual symbol of the degradation and sinfulness of man. Herself the supreme type of vice, she is ultimately the most efficient guardian of virtue. But for her the unchallenged purity of countless happy homes would be polluted; and not a few who, in the pride of their untempted chastity, think of her with an indignant shudder, would have known the agony of remorse and of despair. On that one degraded and ignoble form are concentrated the passions that might have filled the world with shame. *She* remains, while creeds and civilizations rise and fall, the eternal priestess of humanity, blasted for the sins of the people.

"In the eyes of every physician, and, indeed, in the eyes of most continental writers who have adverted to the subject, no other feature of English life appears so infamous as the fact that an epidemic which is one of the most dreadful now existing among mankind, which communicates itself from the guilty husband to the innocent wife, and even transmits its taint to her offspring, and which the experience of other nations conclusively proves may be vastly diminished, should be suffered to rage unchecked because

the Legislature refuses to take official cognizance of its existence, or proper sanitary measures for its repression."

Since that was written, a very small portion of this reproach has been removed. The recent report of a Royal Commission shows with how much good effect.

That a strictly scientific body, in which all European nations were represented, and in which political passion, religious enthusiasm, and intemperate zeal found no place, should have arrived at the same conclusion as the best and wisest of our own profession, and the moralist and historian, is pretty significant proof that the time has arrived to look this grave and corroding evil full in the face, and to extirpate it, as I believe we can, by a careful study and regulation—not of vice and its surroundings—but of a loathsome disease, which is slowly but surely undermining the whole human race, and threatening a moral and physical degeneracy, which it would be simply criminal not to prevent by every means in our power.

ON THE CONCURRENCE OF EPIDEMICS;

AN ARITHMETICAL STUDY; ILLUSTRATED BY THE RECENT BEHAVIOUR OF EPIDEMIC DISEASE IN ENGLAND.

By Dr. BUCHANAN.

[*Read January 8th*, 1873.]

I need not insist, before this society, on the interest that attaches to the question which I propose for consideration to-night; and while we regard it as a problem concerning the distribution and behaviour of epidemics among communities, we shall further admit its interest to the student of the pathology of epidemic diseases.

Hitherto, so far as I know, attempts to investigate the laws under which epidemics prevail together, or refuse to prevail together, have been based on investigations in particular towns or localities, and a number of much disturbing, yet imperfectly understood, elements in the case have always been in the way. We have not known what allowance to make for the difference of the so-called cycles of one epidemic from those of another, nor for the tendency of one disease to attack one part of the community rather than another, nor for the comparative facility with which each of two diseases may be imported, nor for the different ways in which they spread, nor for the different seasons at which they tend to occur. Moreover, we have had, until lately, few data about epidemics except such as related to yearly periods, and a year is too long a time for the purpose of the investigation. And since the quarterly returns of the Registrar-General—the only extended data respecting epidemics in shorter periods—began to be published, only eleven quarters have elapsed, a time very much too short for the investigation of epidemic concurrences in any particular town or locality.

I propose, therefore, to-night to submit to your notice another method of investigation. Instead of regarding the same place throughout a series of epidemic periods, I pro-

pose to regard a number of places during the same epidemic period. I propose to investigate the concurrences of epidemics observed in these places during one quarter year, and to compare the observed concurrences with the concurrences that would, on mere arithmetical grounds of chance, be calculated to occur. With this object, and mainly as illustrative of the method of investigation, I have gone through the ninety-second quarterly return of the Registrar-General—that for the three months, October, November, and December of 1871—and have studied the distribution and combinations of the seven epidemic diseases there tabulated.

On opening this or any other quarterly return, and looking down the columns of epidemics, the first thing that strikes the student is, how seldom two or more epidemics come together in one district as compared with the frequent occurrence of single epidemics in a district. Thus, omitting the metropolis and the larger districts (for the present reason that their figures are not at once understood by the eye), perhaps not more than one place with two epidemics will be noticed for ten that have one epidemic. A very little reflection shows that, unless there be some reason independent of chance to the contrary, this smaller frequency of pairs than of single epidemics must happen as a mere matter of arithmetical probability. Thus, if epidemic small-pox occurs in one-tenth of the districts, and epidemic measles also occurs in a tenth of the districts, then, if there be no other element than chance at work, we should not expect to find small-pox and measles together occurring in the same number, one-tenth, of the districts. On the contrary, as measles occurs in one-tenth of all the districts, assuming still there is nothing but chance in its distribution, it would probably be found in one-tenth of any particular group of districts. It would, therefore, be probably found in one-tenth of that group of districts which had small-pox, and the group which had small-pox is one-tenth of the whole number of districts. The number of districts which we should expect to have measles and small-pox at one and the same time, then, is upon these data one-tenth of one-tenth of the whole, or one hundredth of the whole. In arithmetical terms, if the number of districts be n, if the number of these districts having epidemic small-pox be p, and if the number of these districts having epidemic measles be q, then the fraction $\frac{pq}{n}$ represents the number of districts in which small-pox and measles are to be expected to

concur. This, I repeat, is where no element but chance is at work. If there be in the background any common condition assisting in the production of small-pox which also assists in the production of measles, we should expect to find the frequency of actual concurrence between the two diseases greater than that calculated as the mere arithmetical chance of their concurrence. If, on the other hand, there were in the background some antagonism between the conditions under which small-pox was produced and the conditions under which measles was produced, we should expect to find the frequency of actual concurrence between the two diseases less than that calculated as the arithmetical chance of their concurrence. In either case, it is clear that before any inquiry of a statistical kind can be made respecting community of causation or mutual antagonism, the arithmetical probability of their concurring, as a mere matter of chance, must be determined, and from this arithmetical probability, as a standpoint, further statistical inquiry as to matters that may be in the background must proceed.

I wish now to point out to the Society this great advantage that attaches to the method of inquiry which I am pursuing—that whatever assumption is made in examining the prevalence of one disease is carried on into the examination of the concurrence of diseases. Thus, whatever may have been the so-called epidemic constitution of the period, whether disposing to scarlet fever, or to measles, or to hooping-cough, matters nothing. Whatever may be the cyclical period of each disease, again, is of no consequence, for here we examine them all at once; whatever tendency there may be for one epidemic to occur among different classes of a community or at different seasons is, again, of no consequence; we accept the tendency as comprised in the facts of the several single prevalences of epidemics, and accept it equally when we apply those facts to reckon the probabilities of concurrence at the same period. So, again, how each disease has come to be epidemic has no bearing on the method of inquiry, as any assumption on this point concerning single diseases is carried forward in considering their combinations. It does not even matter what definition we assume of an "epidemic", for, whatever the definition, we reckon the probability of combinations upon the same assumption that we make in noting the epidemic prevalence of each disease singly.

I mentioned in a former paragraph a reason why the cursory reader of the returns would derive less information from

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the figures of large communities than from those of small ones. A further reason, which presents itself to a careful student of the figures, is that large communities often have in some one part of them a hospital in which a particular epidemic disease is treated, and such a hospital would disturb our conclusions by destroying combinations that would have been apparent if such diseases were fatal in the places where they occurred, and where some other epidemic disease that is not sent to hospital is prevailing. The instance of small-pox and hooping-cough among such communities as London or Liverpool may illustrate the influence of such hospitals. In point of fact, I do not find that hospitals of this kind have really counted for much, or that the conclusions derivable from large town districts conflict in any degree with the conclusions obtained from examination of other areas. I can, if need be, give hereafter the figures and calculations on which I base this statement. But meanwhile, in order to avoid what looks like a source of fallacy or confusion, I propose to work the illustrative part of this paper from the data of extra-metropolitan districts having populations below 10,000 apiece.

These districts (they are the *sub*-districts of the Registrar-General) number 1,490 out of a total number of 2,195 districts in England and Wales—about two-thirds of the whole. I find that in the last three months of 1871 the following epidemics occurred in them, and that these epidemics happened, singly or in combination, in the following numbers :—

Occurring	Small-pox.	Measles	Scar-latina.	Diph-theria.	Hooping-cough.	Fevers.	Diarrhœa & Cholera.
In 478 districts, singly	68	24	104	18	53	112	99
In 164 districts, along with 1 other epidemic	35	36	62	9	44	77	65
In 37 districts, along with 2 other epidemics	13	12	23	3	14	26	20
In 6 districts, along with 3 other epidemics	4	3	2	2	3	4	6
In 1 district, in a combination of 5 epidemics	1	1	1	...	...	1	1
Making a total of epidemic occurrences	121	76	192	32	114	220	191

We are now ready to calculate the probability of concurrence of any two of these epidemic diseases in the 1,490 districts. The number of small-pox occurrences was 121 in the 1,490 districts; the number of measles occurrences was 76; then the number of probable concurrences of the two together, all other considerations but the arithmetical one of chance being set aside, is, as we have seen, $\frac{121 \times 76}{1490}$, or 6·1. In six out of our districts, therefore, we should expect to find measles and small-pox epidemic together. Now, the seven above-mentioned diseases can be paired in twenty-one different ways, and reckoning out in the same manner the number of probable concurrences of each pair of epidemics in the three months in the 1,490 districts, we obtain the figures in column A :—

Pairs. Combinations of	A. Calculated number of concurrences.	B. Actually observed number of concurrences.
Small-pox and measles	6·1	8
„ and scarlatina	15·4	14
„ and diphtheria	2·6	1
„ and hooping-cough	9·2	15
„ and fever	17·8	24
„ and diarrhœa	15·5	15
Measles and scarlatina	9·7	15
„ and diphtheria	1·6	3
„ and hooping-cough	5·8	11
„ and fever	11·1	20
„ and diarrhœa	9·6	16
Scarlet fever and diphtheria	4·1	5
„ and hooping cough	14·6	10
„ and fever	28·1	44
„ and diarrhœa	24·3	30
Diphtheria and hooping-cough	2·4	2
„ and fever	4·7	4
„ and diarrhœa	4·1	6
Hooping-cough and fever	16·7	18
„ and diarrhœa	14·5	25
Fever and diarrhœa	28·0	35
Total number of concurrences	250·0	321

Side by side with the numbers indicating the probable concurrences of the several pairs of epidemics, I have placed in this table a column B showing the actual number of concurrences that did actually take place in the three months in the 1,490 districts examined. It will be seen that every possible pairing of epidemics—all twenty-one of such pairs—did actually happen in this period. Next, it will be seen that there is a good deal of general correspondence between the calculated and the observed figures. But, on looking at

particular pairs, we find departures of more or less magnitude of the actual from the calculated degree of concurrence. More or less of such departures are inevitable in any limited number of events, and we have only had 1,490 occurrences with which to deal. Investigation on the present plan can, however, without difficulty be widely extended; and when, by such extended investigation, the true similarity or dissimilarity between the calculated and observed figures has been established, the point is reached at which interest for the student of epidemiology and of pathology begins, and it is upon indicating this point that the object of my paper should properly find its fulfilment.

But, incidentally, I cannot refrain from noticing the possible significance of certain of the foregoing numerical results. If we note the excessive frequency of the concurrence of measles with the other epidemics, of diarrhœa with all the other epidemics except small-pox, of scarlet fever with the "fever" of the Registrar-General; and, on the other hand, the absence of any notable excess of frequency of concurrence between scarlet fever and diphtheria, or between scarlet fever and hooping cough, we shall see points of interest that deserve to be followed up by further statistical investigation of the kind that is now proposed.

Let me now, before concluding, go back to my assertion that it was of no concern what standard we took as constituting epidemic prevalence of our diseases. In the foregoing examination of the quarterly return, No. 92, I have assumed that any one of the seven diseases was epidemic if it caused in any district a mortality of 1·2, or more, per thousand annually; or, in other words, if 3 deaths, or more, from it were found in the quarter in a population of 9,999, or less; or, if 2 deaths, or more, from it were found in a population of 6,666, or less. I have purposely excluded all units of mortality, no matter how small the district population; for, though 1 death in 3,333 represents, of course, the same rate of prevalence as 2 deaths in 6,666, yet, as 1 death might mean only a person sent home to the district after being stricken with his fatal illness elsewhere, or, in any case, could not safely be taken to indicate an epidemic, I thought it best to omit the consideration of such single deaths. If exception be taken to this standard of what constitutes an epidemic, I submit, in the first place, the arithmetical consideration that it can be of no concern, inasmuch as the same standard is maintained for both the calculated and the observed concurrences of epidemics; and, in the second

place, I have, for the sake of any of my hearers to whom this arithmetical consideration does not bring conviction, gone over the districts a second time, taking a wholly different standard of what constitutes an epidemic. Let us suppose that it requires a mortality three times as great as that hitherto assumed in order to constitute an epidemic; that an annual district death-rate must be raised by at least 3·6 per thousand; and that not 1, but 2, deaths may now occur even in the smallest districts without giving the name of "epidemic" to the disease that causes them. We find then, of course, far fewer of our 1,490 districts to have been affected with "epidemics" in the quarter under review, and the following numbers are obtained:—

Occurring	Small-pox.	Measles.	Scar-latina.	Diph-theria.	Hooping cough.	Fevers.	Diarrhœa & cholera.
In 147 districts, singly	29	17	46	2	20	22	11
In 14 districts, along with 1 other epidemic	3	3	8	1	1	9	3
Making a total of epidemic concurrences	32	20	54	3	21	31	14

Examining now, on this new standard of what constitutes an epidemic, the probability of concurrence of any two of these epidemics in the 1,490 districts, and comparing the probable with the actual concurrences, we obtain the following table, in which such concurrences as were actually observed are alone stated in detail.

Pairs. Combinations of	A. Calculated number of concurrences.	B. Actually observed number of concurrences.
Smallpox and scarlet fever	1·2	1
„ and fever	0·7	2
Measles and scarlet fever.....	0·7	1
„ and hooping cough.....	0·3	1
„ and fever	0·4	1
Scarlet fever and diphtheria	0·1	1
„ and fever	1·1	4
„ and diarrhœa	0·5	1
Fever and diarrhœa	0·3	2
Twelve other kinds of combinations in pairs	Each fractional.	Each 0
Total number of concurrences...	8·2	14

The results obtained on this new standard cannot be of the same interest or importance as the corresponding results obtained when a more reasonable standard of what constitutes an epidemic was taken. For the actual number of epidemics on the exaggerated definition now assumed is small; and the probabilities of concurrence of any two pairs is also small; and there is more room in small numbers for deviation of the observed from the probable. Moreover, the probability of concurrence, under this new standard, is so small as commonly to require expression by a figure less than unity; and unity is of course the smallest number of concurrences that can actually take place. But, as showing the arithmetical process to be independent of any question of epidemic standard, the new figures are completely satisfactory. Just as, with the former and more reasonable standard, there was excess in the number of actual concurrences of every sort taken together beyond the calculated number, so with the new standard there is similar excess, only the excess is now proportionately greater than before. And, turning to the possible lessons of the facts, we may now, just as before, note an excessive tendency of measles to coincide with other epidemics. So, too, scarlet fever and fever, fever and diarrhœa, are still seen to go together more than is accounted for by probabilities. We get fewer facts, and we should expect to get fewer, by this investigation of intense epidemics to the exclusion of others; but the facts, such as they are, are in sufficient accord with those obtained by the more detailed examination, when a truer standard of epidemic prevalence was taken.

I have made a number of other notes and calculations from the same quarterly return of the Registrar-General, having reference to the concurrences, probable and actual, of three epidemics, and of more than three epidemics, taken together, out of the seven of which particulars are given in the return. I do not propose to weary the Society with these, beyond saying that out of 35 possible kinds of combinations of three out of the seven epidemics, no less than 25 were represented in the three months, the number of combinations of these 25 kinds having been much in excess of the calculated number, most remarkably in the case of scarlet fever, fever, and diarrhœa. And, on examination of the concurrences of four and of five epidemics in single districts, similar excess of frequency beyond the calculated rate of frequency was found to exist.

THE PERIOD OF INFECTION IN EPIDEMIC DISEASE.

By WILLIAM SQUIRE, M.D., M.R.C.P.

[*Read January 8th and February 12th*, 1873.]

How soon does infection begin? how long does it last? From want of attention to these two points our efforts to check the spread of epidemic disease often fail, even where our knowledge of the laws governing the course of the special infection is full enough to place it almost under our control. Of the two questions proposed the former might be dismissed with an obvious answer as of little importance, while the importance of the latter is as clear as the difficulty of answering it is great; this difficulty may be simplified, and, it is hoped, be made to yield some rules of practical service before the conclusion of the paper. Practical considerations of no slight interest also depend on the first part of the question, the answer to which is not quite so simple as it may seem, but in reality involves some intricate pathological problems, as well as hygienic deductions of the greatest importance.

Effective limitation of disease, in epidemics having a most marked influence on mortality especially among the young, must greatly depend on the recognition of the fact that *infection begins as soon as the disease*, so that our first question is necessarily pushed back to "when does the disease begin?" If the answer be, upon the development of the more prominent and more characteristic sign by which the disease is known—be it rash, hoop, cough, or diarrhœa (and this will always be the popular response)—then, in order to meet the popular notions of disease with warning and direction that shall be generally useful and intelligible, we must let it be generally known and understood that infection is to be guarded against while a person is only sickening for diseases such as small-pox, measles, mumps,

hooping-cough, diarrhœa, cholera, nay even diphtheria, and be prepared to give authority to the somewhat startling announcement that "diseases are infectious before they are developed." The incubation period, as it is called, of epidemic diseases affords us the means of translating this paradox into a scientific position; a correct and definite idea of the first steps, the ingress, of these diseases lies at the root of this question, and merits a full investigation.

The incubation period is to be divided into the two distinct and separate stages of *latency* and *invasion;* a division admitted indeed and applied vaguely to the beginnings of these diseases, but not with that definite and scientific precision which is to be sought for in each special disease of the class, and which must be reached before such general conviction as is alone conducive to useful action can result. Excepting mere conveyance, diseases are not communicable by personal infection until they have themselves been received by the infecting person, nor when received are they immediately communicable, unless certain local effects from contagia, coming before any general illness and even without being followed by any, may give rise to infection, for it may be stated generally that an interval must elapse before infection received can be again propagated. This period, of longer or shorter duration for different diseases, nearly without symptoms and probably non-infectious, is the stage of latency. The stage of invasion commences with the earliest symptoms, and is characterised by the occurrence of fever; at this time the general symptoms may so far predominate over the local as to make it difficult to name the special disease present, but the degree and duration of the fever are aids to an earlier certainty than could be derived from the local symptoms alone, while all taken together afford a timely warning of the infection to be guarded against.

The latent period may either be absent, or be prolonged within certain limits varying for each special disease; to ascertain these limits is necessary to an effective quarantine. The characteristic of this period is its variability, and it is to this that the wide divergence observed in the time of incubation of certain diseases is owing.

The period of invasion is more fixed and nearly constant for each special disease; to recognise the commencement of this stage is necessary to the successful isolation of persons already affected by disease, so as to prevent its extension to others. In the invasion stage the symptoms have ap-

peared, sometimes obscurely and in a slight degree only, yet with greater accuracy in our noticing their course, and with our improved means of recognising them, especially with the aid given by the thermometer, we ought to know *definitely* when the stage of invasion has *commenced*, and to bear in mind that the possibility of multiplying the germs of disease, of propagating and diffusing them to any extent, has from that moment commenced also.

The incubation period, though divided in this way, may still be usefully spoken of as covering all the interval from the beginning of infection to the full development of the disease. A fixed time can more readily be assigned to it as a whole, not without some of the uncertainty that still attaches to its essential divisions; for, what is spoken of as the stage of invasion in a particular disease by one writer, might by another be included in the course of the disease, or, more frequently, be overlooked or lost sight of in the period assigned to incubation. This happens even in so familiar an instance as that of measles, where the well-marked catarrhal stage—the "sickening for measles"—is reckoned in the incubation period, though recognised medically as the invasion stage, and properly treated as an important part of the disease itself. There is a similar stage in the early part of hooping cough, of even longer duration, to be presently considered, when it will be shown that this early stage in both diseases is infectious, just as it is known that the early stage of typhus is infectious. Indeed, what in these slighter ailments is called the stage of invasion, and generally included in the incubation stage, is always in the more serious malady, typhus, looked upon as an integral part of the disease itself, the character of the four days' illness, and the extreme prostration preceding the appearance of the rash of typhus, precluded the possibility of this part of the disease being spoken of as merely "sickening" for an illness; yet this, the popular term for invasion period, is applied to small-pox during four days preceding the eruption, even when, for the two days before the appearance of the spots, the feeling of sickness is so great that the patient cannot stand, the back-ache more intense than is known to any other condition of the human frame, and the temperature of the body raised to 105°, or as high as in any illness, however severe.

This stage of *invasion*, then, differing in each different disease, but uniform within moderate and ascertainable limits for the same disease, may be termed *the constant*

quantity of the period of incubation. It seldom commences immediately or directly upon exposure to its exciting cause, and often at a surprisingly long, though at a remarkably uniform interval; this is the period of latency. The latent stage not only varies for each different disease, but according to the different modes by which the poison of each is conveyed into the blood, possibly also as to the amount and intensity of the poison introduced. Some variation may at times be traced to differences of susceptibility or resistance, depending on the state of health of the individual exposed. This, however, with one exception, modifies but slightly the liability; for any advantage conceded to robust health on the score of greater resistance, is certainly counterbalanced by a greater susceptibility: the one exception being, where a previous attack has conferred a comparative immunity.

The circumstance affecting most definitely the way in which the disease will appear, is the mode by which the poison is introduced into the system; the shortest interval is observed after direct injection into the veins, or into a visceral serous cavity; absorption from intentional inoculation, or from an open wound, also affords a ready inlet to some of the poisons under consideration. Of these, some have a very rapid effect when drawn into the lungs, others are a definitely longer time in acting by inhalation than by inoculation; they generally act more quickly when inhaled in respiration than when ingested with food and drink. Germs swallowed in water are not very likely to enter the air passages, while of those inhaled, some are likely to be also swallowed; in either way it is possible that some may be detained on well protected epithelial surfaces, and for a day or two be producing no more effect on the destined sufferer than if they were left in an infected chamber that he had not yet entered. Allowance must be made for these conditions even when the precise source of infection is known and the duration of the exposure limited, a rare concurrence; consequently, some uncertainty must always attach to the period of latency in a disease commencing from infection; this only, however, may be certainly predicated of it, that the incubation period will be longer than when there has been direct inoculation.

Direct inoculation of the virus of any special disease affords us an opportunity of observing with exactitude the time occupied from the initial processes to the fully developed disease; a slight difference even here is observable in the first stage, and a question arises as to how far the local

multiplication of the inserted germ may go on before such general disturbance of the system is set up as enables us to say that the disease in question is produced. This process, nevertheless, furnishes us with the best analogue to what must take place in most of the diseases to be inquired into, and as it has been largely practised in inoculating small-pox the typical disease of the class, and is every day observable in vaccinia, with still better facilities for comparison with what takes place in allied diseases, a short description of these phenomena will begin the second part of this paper. We may notice now, that though the period of latency is here reduced to a minimum, yet that an incubation period of some days is requisite for the development of the disease, even though we subtract the last day or two of the process and prefer to consider that as part of the disease itself. Exactly the same thing is observable in measles; though the exact time when the poison was introduced may be uncertain, it must be certain that it was introduced days before the full characters of the disease become obvious, for these days are necessary to the development of the initial processes of the disease.

It may be affirmed, therefore, of some diseases, that *they cannot show themselves* except after a certain interval, "the stage of invasion"; so that if to this there be added, as often there must be, an uncertain period of latency, then, consequently, the incubation period of these diseases must be a long one. On the other hand, without assuming that some diseases spring forth at once ready armed with all their dire properties in full display, yet it is seen that the different processes by which this virulence is acquired may be gone through with extreme rapidity, and of those, it may be affirmed that the period of incubation may be short.

Small-pox, vaccinia, measles, rubeola, mumps, varicella, typhoid, typhus, and from one point of view, hooping-cough, represent diseases having a long incubation period. Relapsing fever forms the connecting link with the division having a short incubation period, where we find besides relapsing fever, scarlet fever, diphtheria, plague, cholera, yellow fever, diarrhœa, influenza, erysipelas, and if strictly investigated, hooping-cough, which combines some of the leading features of both classes.

A long incubation period is generally followed by a very definite illness terminating in a crisis, and, excepting debility, with sequelæ neither very prolonged nor very definite, the infection ceasing at a comparatively early period of

convalescence. A short incubation period commences a sudden morbid disturbance, having either a long or a short process, very liable to relapses, and to prolonged or definite sequelæ, infection persisting far into convalescence. It follows that infection is spread most at the end of these diseases, generally from impatience of their last lingering effects; and at the earlier, or even the earliest part of those at the head of the list, from disregard of their premonitory symptoms.

Exact data as to the origin of upwards of forty cases of measles have been collected, showing to what extent the time of incubation may vary; many of these instances illustrate in a striking manner how constantly this disease is propagated before the rash appears, and consequently how hopeless is the effort to prevent its spread among those who have been in close contact up to the time of the eruption. Where no such previous communication occurred, an effectual quarantine has been maintained, as shown by cases 22 and 23; these were brought home at the height of the rash of measles, but kept apart from the other children of the family, none of whom took the complaint.

Hooping-cough is brought into this group more because of the frequency with which it is communicated in its early stages, than from any valid claim it may have to a long period of incubation. The stage of invasion is often considerably prolonged, nor need we dispute as to the claim this stage has to be considered, in the absence of the hoop, an essential part of the disease itself, so long as we recognise that it is a serious and highly infectious part of it. The remarkable association of hooping-cough with measles also gives it a right to some notice here; statistics on a large scale show that this connection is the reality that partial observation has frequent occasion to remark: a close parallelism is also observable in their natural history, so that sometimes a doubt may remain whether, in a particular instance of impending illness, the one or the other is to result.

Measles and hooping-cough may be regarded as the two members of totally distinct groups or circles that approximate the nearest to each other, but as individually separated by a wider divergence than exists between the different members of the same group; they exhibit a mutual interference in their development, so that if contracted together either the one or the other is delayed; a mutually intercepting influence is also noticed with regard to hooping-

cough and varicella; while with the more nearly allied diseases of the first group it would seem that some of their initial stages, instead of intercepting each other, could be accomplished together. When a child has been exposed to the infection of both mumps and measles, the measles having the shorter incubation period, is the first to appear, mumps showing itself about ten days after the subsidence of the measles, or in a time but slightly, if at all, in excess of that sometimes occupied in the development of mumps when there has been no intervening illness.

These diseases are frequently separated for us by a process of clinical analysis, and shown to be distinct whatever be the degree of affinity between them. Where, as in cases 18 to 21, a child who has previously had hooping cough takes the infection of measles from another suffering from the mixed complaint, the former communicates measles only to one who has never had hooping cough; even those susceptible to hooping cough, as cases 27, 28, may contract measles only from one suffering from this complication, if a moderately careful separation be maintained after the termination of the eruptive disease. During the earlier part of the concurrence of mumps with measles, even where some symptoms indicative of mumps are already present, up to the completion of the eruptive process, measles only is communicated to children susceptible of both; a child having received the infection in this way will propagate measles only. Persons who have previously had mumps when exposed to the mixed complaint after, or at the time the mumps is most prominent, will take and give measles only. Where the exposure is the same, but the susceptibility reversed, mumps only will be communicated.

Infection, commencing at the very beginning of diseases of the first class, and probably most intense at their height, continues with great intensity during the earlier convalescence. In small-pox, measles, mumps, and varicella, for a fortnight after the eruption the greatest care is requisite to guard against their spread; but many instances point to the infection of these diseases of slow ingress disappearing in a comparatively short time afterwards, so that three weeks may often suffice to terminate the persistence of personal infection—this, in small-pox, corresponding to the disappearance of the last pustular crusts. No such limited time can be assigned to some diseases of the other class. Hooping cough, contagious before its characteristic sign is developed, probably ceases to be infectious before all traces

of spasmodic cough are lost; but scarlet fever continues to be infectious long after all remnants of local morbid action are removed, so that personal contagion may persist for nine or ten weeks from the commencement of an attack. The shortness of the incubation period, and the suddenness of ingress, in scarlet fever, diminishes the chances of its being unexpectedly conveyed, and enables its progress to be checked by a timely separation of the sick. The long incubation period of other diseases favour their unexpected dissemination. In one of them, measles, it would not be too much to say that one-half of all the cases met with are contracted during the premonitory or catarrhal stage of that complaint.

To limit the extension of most infectious diseases, an early method of recognising them is necessary. A close parallelism is presented in the earlier stages of most of them, and this has its exact counterpart to what is observable during vaccination. By a closer study of the one we learn to appreciate the more delicate indications of the others in their ingress; and not until we recognise these shall we be able to stop the spread and limit the ravages of many of the most fatal diseases. On this account the invasion of the more readily inoculable infectious diseases should receive careful study.

There is another reason for entering minutely into this part of the question. By inoculation we eliminate what, for all diseases except small-pox, is the uncertain period of latency, and there remains the fixed time in which the disease will appear. The first half of this may generally be attributed to the true incubation stage; the latter half to the stage of invasion. In this latter part febrile movement is progressive and continuous, so that it cannot be stopped or averted; products of the diseased action are formed, and infection begins. This point is marked by elevation of temperature, easily recognised by the thermometer, and often preceded by an equally recognisable thermometric depression; until this time the disease is only incubating, it is not yet infectious, and might possibly be averted or aborted. This really occurs oftener than is supposed, and is one mode of escape from the diseases we must frequently encounter. It is observed sometimes after slight exposure to infection, when premonitory symptoms appear and clear away again without giving rise to the illness; or, when, by gradual exposure reducing susceptibility, a temporary immunity is procured, affording no protection against the liability to subsequent infection.

Inoculation of diseases such as small-pox and measles shows us that a large proportion of the time of incubation is in reality occupied by certain initial processes necessary to the development of the disease itself, so that it would seem to be impossible for those diseases to appear for a week, or more, after infection; yet, while measles may originate in almost as short a time from infection as from inoculation (viz., on the seventh day, the latent period here being reduced to a single day), small-pox requires four days more if received by inhalation than if by inoculation; for the general illness begins on the twelfth instead of the eighth day, and the rash does not appear till the fourteenth, the latent period occupying, in this case, always four days.

A close illustration, not only of what takes place in inoculated small-pox, but of what is to be observed in the ingress of all diseases of this class, is afforded by vaccination. Immediately after the insertion of the virus some effect is produced; an acceleration of pulse, and sometimes a temporary elevation of temperature is noticeable by the end of the first, and not later than the commencement of the second day, when nothing but a slight discoloration of skin remains at the points of insertion; next, a certain lowering of the vital powers, characterised by a depression of temperature, is always observable at the third or fourth day, whilst, at the points of puncture, papules are developed into vesicles, unless, at this stage, the vaccination fails and the disease is aborted. The earliest appearance of papules has been observed at the end of forty-two hours; they are always present on the third day; on the fourth, the papules become vesicular, when local and sometimes general irritation commences; this decreases as the vesicles become more developed; sometimes these are moderately full and pearly by the fifth day, sometimes a day or two later; as they fill, any slight irritation of the skin or mucous membrane subsides, and the general irritability gives place to quiet and ease. This lull is found to occur at the beginning of other allied disorders; the temperature hitherto less disturbed than the pulse, and respiration now shows a pretty constant elevation, more marked at night; indeed, by the sixth day signs of constitutional disturbance are often very obvious, the more prominent symptoms of this being bronchial irritation and coryza, not unfrequently accompanied by palpable enlargement of the glandulæ concatenatæ, in the neck; any fulness of the axillary glands in

well nourished children escapes detection. On the seventh and eighth days the pulse and respiration are quickened, the temperature only moderately elevated. The vesicles fill with lymph on the eighth day; and then, as if all the activity of the virus were concentrated here, the signs of bronchial and other irritation subside, and the child's state of placidity and content differs only from the healthy state in that it rather conduces to repose than to activity. This is of no long duration; for, on the maturation of the vesicles they are surrounded by a red areola, and that night, or before the ninth day is over, the process is terminated by a febrile exacerbation of great violence though of short duration. After this, the areola fades in the centre; the vesicles begin to dry, not without secondary vesicles having sometimes accompanied the fever; the arm is tense and painful for a day or two; but that the diseased action has completely terminated is shown, not only by the subsidence of all increase of pulse and temperature, but by a reduction of the latter below, and often considerably below, the normal. Here we have four days for incubation, and four days for the invasion, or initial stages of the disease. During these last four days contagion begins; for, from the earliest vescicle the disease can be reproduced, while from vesicles of the sixth and seventh days it may be so certainly propagated that the only objection to the practice would be the smaller quantity of lymph procurable at the time. A variation is observed in the study of vaccinia of one day of acceleration, and of more than one day in its retardation. This is not noticed of variola, when, possibly, a greater intensity of the poison may overcome this slight tendency to variation. It is, however, noticeable in measles, where the ingress has been studied with much the same exactitude, and seems not to be without its counterpart in other of the diseases under consideration.

This variation is in all cases found only in the first part of the process. What usually occupies four days may be completed in three, or more frequently, perhaps, prolonged to five. Sometimes the first effects of the inserted virus follow with remarkable celerity, or a delay occurs before the first disturbance is noticeable. In this case it may be inferred that a short period of latency has been possible. Towards the close of the incubation period, a sort of strife seems to be carried on between the self-multiplication of the poison and the efforts for its elimination. These efforts may succeed before the poison has increased, and the disease be

aborted (which seems alone possible during the wave of low temperature described), or may result only in prolonging the contest; in which case, though the wave of low temperature may be continued beyond its usual limits, there is no subsequent interruption of the diseased process, which thus, from first to last, is an elimination of itself *in its own form and manner*; which self-elimination, the sign of the disease, is the very source of infection, and the basis of the axiom that infection begins as soon as the disease.

The phenomena following the inoculation of small-pox have been traced for us by a master hand. Dr. Gregory, noticing the earlier local effects more than the systemic, begins his description with the second day, when a shrivelled stain (where absorption has taken place from the point where the virus was introduced) is all that can be seen. On the third day there is a papular elevation; this on the fourth is vesicular, and local irritation commences; on the sixth there is stiffness in the axilla, and proof of the poison producing a more general effect. "Occasionally," he says, "on the seventh, but oftener on the eighth day, rigors occur, "accompanied sometimes with faintishness, sometimes with "pain in the back, headache, or vomiting. The patient "complains of a disagreeable taste in the mouth, and the "breath is offensive, soon after which the eruption shows "itself." The identity of these two last days of illness with the two days preceding the eruption in ordinary cases of small-pox taken by infection is thus placed beyond doubt, nor need there be much difficulty in considering these two days as part of the disease. At the end of the last century, when inoculation was the common practice among children, they were removed to a house provided for the purpose as soon as the sickening began, and before the eruption appeared, so as to prevent small-pox being set up in their own houses. Our greatest authority of the present day, Mr. Marson, says of small-pox, "Most likely, it is communicable from the moment when the initiatory fever begins. It may be given by the breath of the patient before the eruption has appeared on the surface of the body."

Dr. Thomas Jones, in the *St. George's Hospital Reports* for last year, pp. 230-40, gives a most instructive account of how small-pox was propagated in that hospital through the medium of bed-clothes and linen used by a patient for days preceding the eruption; this happened again on five separate occasions, and the disease communicated to twenty-five patients in the hospital before the mode of conveyance

was detected, and the further spread of the outbreak stopped. In all these occasions the cause was traced, by counting fourteen days backward from the eruption in the different cases, to the removal of linen from those suffering from an attack; its redistribution to the wards showed how, in ordinary washing, heat is seldom applied at a sufficient height, or for a sufficient time, to destroy infection. The original source of infection was a woman who had been out of the hospital (and so possibly exposed to contagion) exactly twelve days before she was taken ill, and fourteen before the rash.

These cases, however illustrative of the axiom that "the time for taking the disease to its appearance on the skin is never longer than fourteen days," do not afford precise data for its confirmation; our conviction is based more upon common experience, and the rarity of any valid instance to the contrary being brought to notice, than upon the recorded facts of cases that had originated from a definite source of infection where the limits of the exposure were accurately known: it would be satisfactory to receive the particulars of such cases from our small-pox hospitals; any illustration of variation thus collected would not be without both scientific interest and practical utility. Dr. Gregory* has published an instance of twenty-one days' incubation of small-pox from a definite exposure; for the short period of ten days he gives very exact details of a young medical friend accompanying him to the Small-Pox Hospital on a Thursday. He soon after became languid, and his appetite fell off. On Saturday, in the ensuing week, rigors supervened, and, two days afterwards, the eruption of small-pox." These and seven other cases present a striking analogy to what will presently be seen in measles.

We may conclude that small-pox begins in most cases twelve days after infection, and seven or eight days after inoculation; so that it would seem to be almost an impossibility for small-pox to appear in less than twelve days from exposure to infection, or in less than eight days after the reception of the virus in the most direct manner, the

* "An Essay on the Periods of Incubation of the Various Morbific Germs: addressed to the Central Board of Health, by George Gregory, M.D., Physician to the Small-Pox Hospital." The *Cholera Gazette*, Jan. 28th, 1832, No. ii, p. 60. This has only come under notice while revising the present article for the press.

latter being the shortest time in which the disease can be developed, even when the period of latency is reduced to a minimum. The latent period being, in all ordinary circumstances, four days, we have four days for incubation, properly so called, and four for the initial phenomena of the disease.

The infecting poison of small-pox so abundantly produced during the height of the disease, and of such intense activity then, does not seem to attach itself to the individual for long after the pustular incrustations clear off at the end of the third week, though the infection will cling to the clothes and furniture for a long time; it may lurk in some parts of the chamber for two years, as one case that came under my own observation seemed to show, and might be preserved in the dried crust indefinitely.

Small-pox also presents the typical example of what is seen in some other diseases, for instance, scarlet fever and hooping cough, viz.: that persons suffering from a second attack, however mild or however modified, may communicate the disease, in all its virulence, to others who are susceptible. A most significant illustration of this is given by Mr. Marson, in the case of a lady who, twelve days after meeting with a person infected with small-pox, has an illness, with delirium, but no eruption; a sister confined to the house with her, is taken ill twelve days afterwards, and has confluent small-pox. Something analogous happens sometimes in measles, when convulsions and lung complication have prevented the appearance of the rash, yet infection is propagated; in such a case protective effect is produced if the patient recovers.

Measles has a shorter time of incubation than small-pox: the rash will never appear until a week after infection, it will often be deferred until a fortnight; the usual interval is ten or twelve days. Measles has been inoculated, then the interval was seven days; now the initial processes of the disease occupy six days, of which the last four constitute the well-known catarrhal stage of measles, so that we have here but three days of incubation, even if we include the initial fever, which terminates the incubation stage. This period of incubation is prolonged for a day or two, not without symptoms, when the disease is taken by infection, and then from two to four days of latency must often be added, so that a child cannot be said to be free from the fear of taking measles, and consequently of giving it to others, for a fortnight after being exposed to infection. The longest interval met with has been eighteen days.

The first of the following cases, affords the shortest interval from infection of measles.

CASE 1.—A girl, 12 years old, who had been away from her family since Christmas (certainly for three weeks), is brought home on January 24th, 1865, when her next sister and a younger brother are both ill with the measles, and have the rash upon them; two days after her return this girl has headache, and the initial feverish disturbance, with quickened pulse on the 27th; catarrhal symptoms on the 28th, the full rash on the 31st; seven days only after coming to the infected house.

CASE 2.—A girl, aged 11 years, sister to the above, was slightly febrile on January 17th, she attended an evening party on the 18th, had coryza on the 19th, rash on the 22nd; the illness was at its height on the 23rd and 24th, with serious short complication; time uncertain.

CASE 3.—A boy, aged 7, constantly with his sister, case 2, is not noticeably ill till the 22nd, with rash on the 24th, at its acme on the 25th; time uncertain.

CASE 4.—A girl, aged 9, sister to the above, at home throughout the illness of the others and frequently in the sick room; slightly febrile on January 27th, pulse not quickened till the 29th, full rash on Feb. 1st; ten days.

So that this child, Case 4, who was with the others throughout the illness, does not take it so rapidly as her elder sister, who is brought fresh into the infection when at its height.

Cases 2 and 3 probably contracted the disease at the same time from an unknown source of contagion.

CASE 5.—A boy, aged 7½ years, attends a school daily, when on Oct. 24th, 1868, a boarder is complaining of illness, which is known to be measles on the 26th. The boy, case 5, was not at school on the 25th and 26th; he went for his books only on the 27th, and then stayed away; on the 31st he vomits, seems pretty well next day, but is ailing on November 2nd, and has the full rash on the 6th; consequently, six days before any symptoms, and twelve to the rash.

CASE 6.—A brother, aged 5 years, with exactly the same exposure on October 24th, had the initial fever on November 2nd, high fever on the 6th, full rash on the 7th and 8th; time, thirteen to fourteen days.

CASE 7.—A girl, aged 6 years, sister of these last two boys, was removed from the house in which they were nursed on November 5th, and was not in the room with her brothers after November 4th. She has the rash on November 15th and 16th, or, after twelve days.

CASE 8.—A boy, aged 1¾ years, brother of the above, removed on the same day as his sister, November 5th, and slept in the same room with her; is not noticed to have any symptoms of illness till November 15th, when the pulse was 116 and the temperature 99 degs. at night. He has the rash with convulsions on the 18th; this is on the fourteenth day after separation.

CASE 10.—A boy, aged 12 years, leaves school, where cases both of mumps and measles had recently occurred, for the Easter holidays on March 20th, 1869, and goes to Brighton. He seemed dull on the 27th, returns home on the 29th, and has the rash of measles on the 30th, on which day by noon he is removed to another house; ten to twelve days; ten days after this he had mumps.

CASE 11.—A boy, aged 6½ years, who was with the last case from March 26th to the 29th, and is watched rather closely lest he should have the measles, has the initial fever on the night of April the 4th,

and, after a slight subsidence, the usual symptoms on the 7th and 8th, with full rash on the 9th and 10th; again twelve days from greatest exposure.

CASE 12.—A girl, aged 5 years, sister of the above, is one of three children, whose chief exposure to infection arose from the boy, case 10, having spent one night in their house while throwing out the rash of measles, March 29th and 30th; she has premonitory symptoms on April 4th, some cough came before that, and the not unusual temporary lull in the symptoms on the two following days of incubation, coryza on April 8th, and the full rash on the 11th and 12th; twelve days' interval.

CASE 13.—A boy, aged 4 years, brother to the above, exposure the same, has cough on April 9th, and the full rash on the 13th; fourteen days' interval.

None of these children took mumps on this occasion, though the boy from school, who brought measles into the house, also suffered with the mumps ten days afterwards, Case 1 of mumps.—The eldest boy in this family was much with him during the incubation, but not after the eruption of measles. They all (Cases 6 to 12 of mumps) passed through that ailment in the following year.

CASE 14.—A younger sister in the above family, at first not much with the other children, sleeps with them on the nights of April 9th and 10th; she begins to cough on the 22nd, and has the rash of measles on the 24th and 25th; probable interval fourteen days.

CASE 15.—A girl, aged 6 years, much with a companion until May 10th, who, a few days after is ill with measles, complains of fatigue on the 15th, and from that time remains at home; she becomes febrile on the 19th, with sore throat, fulness of the gland at the angle of the jaw, stiff neck; next day there is less fever, but the glandulæ concatenatæ are enlarged; there is cough on the 21st and 22nd, coryza on the 23rd, with less cough, and the first signs of the rash. This is fullest on the 25th; on the 26th there are severe pulmonary symptoms, relieved by stimulants, but persistent with complete defervescence on the 27th; fourteen days' interval.

The following cases not only illustrate how measles is propagated during its catarrhal stage, but how a sort of clinical analysis is often performed for us, separating hooping cough from measles, just as in some other cases we shall find mumps separated from measles.

CASE 16.—A girl, aged 4 years, is taken to see a brother who is ill at another part of London, on February 19th, 1872. She only stays half an hour, as her brother is found to have measles, with the rash fully out. On March 1st, ten days afterwards, for it is leap year, the little girl sneezes; on the 2nd she has headache, and feels ill; is quite ill on the 3rd, and has the full rash of measles on March 4th; fourteen days afterwards.

CASE 17.—A boy, aged 8 years, brother of the above, exactly the same exposure, does not complain of illness until March 4th, next day he seemed better till evening, when spots of measles were perceived; on the 6th and 7th the rash was full; here a whole fortnight intervened, or sixteen days from exposure till the rash.

CASE 18.—A girl, aged 6 years, ceases to attend school on November 25th, 1871, because she seemed to be unwell. On November 28th or 29th she begins to have a cough, suspiciously like hooping-cough. On

December 4th she has the first sign of measles, and the full rash on the 6th and 7th; twelve days after her last attendance at school. The subsequent course of her illness makes it probable that she contracted hooping-cough at the same time.

CASE 19.—A girl, aged 6 years, visited the above girl on Sunday, Dec. 3rd, 1871, the cough was then the only symptom, and no signs of measles were visible till the next day, after which no communication was continued between the two houses, as the children of this family, though they had had hooping-cough had not had measles. On December 14th she was seriously ill, but had not the full rash of measles till December 17th or 18th; fourteen to fifteen days after.

CASE 20.—A boy, aged 4 years, brother of the above, exposure the same, had rash of measles on December 14th and 15th, twelve days from exposure; neither of these children had any subsequent cough.

CASE 21.—A boy, aged 1 year, who had been kept in-doors with these children for two or three weeks, after a day or two's illness, has the rash of measles on December 26th and 27th, ten days after his brother and sister. This child, not having had hooping-cough, has no subsequent cough.

The next case presents a very clear example of measles being infectious during the earlier stages of invasion, and before the rash appears.

CASE 22.—A girl, aged 10 years, coughs and is febrile on February 20th; she has coryza and epistaxis on the 23rd, with the rash of measles on the 24th, attaining its height on the 25th, and subsiding, with complete defervescence, on the following day. A little boy had come on a visit to the girl's home on February 5th; these children occupied the same room at night from the 9th to the 12th; the boy is first noticed to be ailing on the 13th, and returns at once to his own home; he has signs of measles on the 15th, and the full rash on the 16th and 17th. He must have communicated the disease to the girl two or three days before himself throwing out the eruption. The interval from his leaving to the height of the girl's attack of measles is twelve days; had they not been separated on February 12th the interval from rash to rash would have been set down as eight days only.

The highly infectious nature of the catarrhal stage of measles is doubtless the cause of this disease so frequently spreading through schools and families. With every desire to prevent extension of the disease the separation is effected too late; the next case is an instance of this.

CASE 23.—On March 2nd, 1868, a boy at a day school is confined to his class-room because he was dull at his lessons; next day he is too ill to come at all, and the day after has measles. On March 12th, several boys who met in this class-room were away from school ill; they had the rash of measles from the 14th to the 16th of March.

On the other hand, where children have been kept apart during the earlier stages of measles the limitation of the infection is possible, even where, as in the next cases, it is brought into a dwelling at its acme.

CASES 24 and 25.—Two girls of 14 and 11 years old, are sent to school, January 13th, 1872; they have coryza on the 27th, and are brought home with the rash of measles fully out on the 30th. There are five younger children in the family, three of whom remain at home from the

time the sisters arrive, the other two being sent away that a fair quarantine may be established; none of these children take the disease, and three weeks after it is over they mix freely with their convalescent sisters.

Instances of infection from a short exposure, both the limits of which are known, can never be so numerous as those from a longer exposure of uncertain duration; nor are they of such crucial value in determining the period of incubation as might have been expected, the rule being that the interval is longer after a single definite exposure of short duration, than where the exposure has been more continuous. The example of shortest interval (Case 1) resulted from a continuous exposure, the earliest limit of which was fixed. The instance of longest interval, presented by a case where the latter limit only of a continuous exposure is known, is the following:—

Case 26.—A boy, aged 6 years, one of a family of three children, attends a day school until November 17th, 1871, when he is removed from fear of measles and hooping cough, both prevailing among the scholars. He remains at home for some days as he seems to require rest; there is no other family in the house and no visitors. On November 26th he walks to the park, a very short distance, and is obliged to return, he is so soon tired; next day he seems rather better, but is chilly on the 28th; he coughs and is sick on the 29th, and continues ill with fever and loss of appetite till December 4th, when he is delirious at night; next day he has the first signs of measles, and the full rash on the 6th and 7th. The interval from exposure to the rash was here eighteen days, but there was reason to suspect a complication with hooping cough.

Case 27.—A boy, aged 4 years, brother of the last and much with him until December 4th, sickens on the 10th, and has the rash of measles on the 14th, but does not take hooping-cough.

Case 28.—An infant, aged 8 months, of this family, not much with the other children, also sickens on the 10th, and has the rash on Dec. 14th.

The two younger children have hooping-cough two years afterwards, when the elder boy escapes.

Case 29.—A boy, aged 6 years, at a large day school, where are both measles and hooping-cough, is feverish on the night of March 26th, 1872, and remains at home; he coughs on the 28th, and has the rash of measles on March 31st.

Case. 30.—A boy, aged 4½ years, not at school, brother of the last and with him from March 26th, has the rash of measles on April 7th, with rapid defervescence on the 8th, and no subsequent cough; probably having taken the infection of measles from his brother while he was sickening. The elder boy was seriously ill for the four days preceding the rash, with delirium and convulsive dyspnœa: again, after the rash, he was dangerously ill with extreme dyspnœa and spasmodic suffocative cough until 12th, with clubbing of the fingers and dusky face and lips; the lung complication not being proportionate to the distress suffered. On recovery at the end of the month, the finger ends became flattened at the sides, and the nails for a time arched. This boy had previously had hooping-cough. His father suffers from asthma.

The following cases were complicated with subsequent hooping cough.

CASE 31.—A boy, aged 6 years nearly, the eldest of four children in a poor family, commences, by school-board authority, to attend day school on March 11th, 1872; several children in his class have coughs; he begins to cough about ten days afterwards, and had slight symptoms of cold, but continued his attendance at school up to March 28th, when the cough, already spasmodic, almost amounted to a hoop. He was quite ill on March 31st, and had the full rash of measles on April 2nd and 3rd; he was rather slow in recovering his appetite and strength, but seemed pretty well on April 20th, soon after which his cough became more noticeably spasmodic, and he suffered the extreme effects of hooping-cough, from which he died on June 2nd.

CASE 32.—A girl, aged 5 years, sister of the above, at the same school, in a different class, discontinued attendance at school on March 28th, when she has coryza, but less cough than her brother; on April 2nd she appeared lively and well, but had a quickened pulse and a temperature of 102½ degs.; this exceeded 103 degs. by morning, and was more than 104 degs. in the evening, exactly 104 degs. next morning, April 4th, by evening it slightly diminished as the rash of measles appeared. There was a full rash on the 5th and 6th, with complete defervescence on the 7th.

CASE 33.—A girl, aged 3 years, another sister, has the first febrile disturbance, and some redness of the conjunctiva, on April 5th, with slight subsidence on the 7th and 8th, coryza on the 9th and 10th, full rash on the 11th, and defervescence on the 12th. This child probably took the infection from the others between March 28th and April 2nd.

An infant in this family, a boy, has slight cough, with sickness, on the 5th of April, disappearing after two or three days without rash. He begins with hooping cough on the 4th of May. The two girls showed the first symptoms of hooping cough on April 28th, a week later than the elder brother. Supposing that the elder boy took the infection of hooping-cough with that of measles, at school, and that his elder sister did not, which seems to be the necessary conclusion to be drawn, an illustration is afforded of how a certain proximity is necessary to the conveyance of infection; it is probable that effective ventilation, both in bedrooms and schools, would limit in a great measure, if it did not prevent, the spread of the disease.

The order of infection with hooping-cough shows it to be no necessary consequence of measles, appearing at a definite time afterwards, for here two girls take the hooping cough together, while the one child had measles a week later than the other. Both these inferences are supported by the following cases:—

CASE 34.—A girl, aged 3 years, supposed exposure to infection December 24th, is ill on January 5th, 1872, and has the full rash of measles on the 6th. She begins with hooping-cough on February 3rd.

CASES 35 and 36.—Two girls, aged 5 and 4 years, sisters of the above, became ill January 13th, and have the rash of measles on the 15th and 16th, followed by some spasmodic cough. They have hooping-cough, the elder in March, the younger in May.

The order in which they were exposed to infection

requires to be further noticed here. The youngest girl, the first to begin with measles, seemed to be making a good recovery, when on February 2nd she was taken on to the roof of the house, where there was a garden (the first and only time of her going out during her convalescence); next day she had a more marked cough, and again became febrile, with well-marked symptoms of hooping-cough of some severity, from the 6th to the 19th of February. During this time the little girl was nursed apart in her mother's room, on the birth of a baby she had to go to another room, at first with the nurse and afterwards with the elder sister; this sister has hooping-cough in March. The little girl again goes to sleep at night in her mother's room in April, and before the end of the month the infant, now two months old, gets the cough. The other girl, aged four years, who had been more alone, and slept in a separate room, does not take hooping-cough until May 4th, has high febrile disturbance all the month of May, and dies with symptoms of cerebral disease in the middle of June: so that the hooping-cough was contracted the one from the other, and had no definite relation to the measles, for the infant, who had not measles, took it sooner than the little girl who, having had that complaint, was kept longer separate from the others.

This part of the subject will be concluded with cases of uncomplicated measles in two other families, and two instances of second attacks.

Case 37.—A boy, aged 10 years, at home from school for the Christmas holidays, sickens for measles on January 17th, and has the full rash on 21st. A younger brother sickens on the 26th, and has the rash of measles on the 30th, ten to twelve days after exposure.

Cases 38 and 39.—Two girls, aged 8 and 4 years, sisters of the above, show symptoms of measles on the 27th, and have the rash on the 31st of January, ten to thirteen days after exposure.

Cases 40 to 44.—A boy, aged 7 years, second child in a family of four, was noticed to be ill on July 18th, has coryza on the 20th, and the full rash of measles on the 22nd. An elder sister sickens on August 2nd, and has the rash of measles on the 6th. Two younger children, ill on August 4th and 5th, have the rash on the 8th or 9th. The mother who was in constant attendance upon the children, but had had measles herself when eight years old, felt sick and giddy, with aching of limbs at night, on August 9th, seemed ill as with a cold from the 10th to 12th, and, on the morning of the 11th, found herself covered with rash; this began to fade by night, leaving some irritation of skin, and a feeling of weakness, but no further illness.

Case 45.—A young man, aged 23 years, had measles fifteen years ago, felt giddy on the evening of February 27th; next day he was ill with signs of catarrh, there is rash at night with cough, and every characteristic of measles. On tracing back fourteen days, it was found that he passed the night of February 14th shut up in a close cabin in crossing from Liverpool to Dublin; he returned on deck, and thought he had taken cold in his journey home, where he had but just arrived in time

to take to his bed on the evening of the 28th. The account of the previous attack is quite clear and substantiated by family memoranda written at the time. This attack is in no way shortened, for the rash and restlessness continued till March 3rd, and the fulness of throat and of the glands of the neck till the 6th; moreover, three weeks afterwards, going out in the cold, he suffered smart fever and herpetic catarrh on March 21st to the 23rd.

Most important of the inferences from these are, first, that in second measles whether highly or slightly modified, the period of incubation is unaffected; second, that the stage of invasion is more abrupt, in each of these two instances it is shortened to two days.

Sometimes the ingress of measles is so sudden as to be unnoticed, this will seldom happen in more than one of several children affected at the same time, and probably in none of those infected from the exceptional case, so that all doubt as to the true nature of the disease soon vanishes. The following three cases bearing upon this point occurred in the family of our most accurately observant member.

CASE 46.—A florid healthy girl, aged 6 years, has the rash of measles on February 4th, with no obvious evidence of previous sickness, not even for an hour; there are two other children at home with her who have not hitherto been segregated by day. On looking back a fortnight it was found that these children, a sister one year older and a brother one year younger than this little girl, all carefully withheld from Christmas parties, had been taken to the pantomime on January 21st and to a circus on the 20th. It soon became evident that, with the same exposure, the sister and brother had not taken the infection at that time; they were now carefully watched. The elder girl has headache on February 12th, at night the temperature is high, next day this is normal, but is raised on the night of the following day to 101 degs.; it continues to be high for the next two days with cough; the rash appears on the evening of the 15th, and is full on the 17th, defervescence is sudden and complete on the 18th. The little boy is placed under observation at the same time. A slight elevation of temperature is found on the night of February 12th, next day there is cough; some spots of measles appear at noon on the 15th, and there is copious rash with temperature of 103 degs. at night; on the 16th the rash and all the symptoms are very intense; on the 17th the temperature is reduced to 99½ degs. in the morning after diarrhœa, in the evening it is raised to 102½, with a trace of bronchitis; next day this disappears, and the defervescence is complete: the interval for the last two cases is twelve days.

The two succeeding cases have some features both of resemblance and of contrast to the last; they show that a long interval may be looked for after a limited exposure, and that a continuous exposure tends to shorten the interval.

CASES 49 and 50.—Two little girls, the youngest, aged 4½ years, has the rash of measles on Saturday, March 8th; she had been ailing all the week, and had seemed dull and easily tired in the preceding week; she had been with her sister to a morning performance at a circus much frequented by children on February 22nd. The elder girl, aged 10

[illegible] when seen at noon on March [illegible] seemed to be quite well, except for a slight sensibility of manner; a quick pulse of [illegible] and an elevation of temperature to [illegible] under the tongue—this probably arrived for morning at night it was [illegible] degs. only. Next day both pulse and temperature were normal. It was evident that this child had not taken the infection when with her mother at the circus. She had slept in the same room her mother during the two nights of fever, and besides having been with her mother during the few preceding days, was now entirely confined to the same room: she was without any sign of illness on March [illegible] and [illegible]; on the [illegible] she was lively, but again had a slight rise of pulse and temperature; on the [illegible] she was sick in the morning; next day the pulse was [illegible], the temperature [illegible] degs.; her appetite continued good until the [illegible] when the temperature rose to [illegible] degs., and the rash appeared in the evening; on the [illegible] there was full rash after a temperature of [illegible] degs. had been reached; next day there was the usual evidence of temperature, with complete deferveseence on the following day.

From the care those children always receive, from the limited number of their visitors, from the fact that the elder only of the two goes to school, and that the younger is always with her mother, who even takes her for her walks, no doubt remains as to other sources of infection; tracing back a fortnight from the appearance of the rash of measles brings us to this visit to the circus, the one amusement with which these children had been indulged. An illustration of what is noticeable in the previous cases and must frequently happen, is seen in only one of these children taking the disease, though they both had the same or nearly the same exposure; fortunately, many risks of this kind are escaped. How soon the symptoms developed by the younger child could have imparted infection to the elder is uncertain; allowing four days for the period of infection before the rash appears as the limit, then, in the second case, the rash came on the twelfth day, but as the exposure was much greater at the height of the rash, namely, on March 8th, and the two subsequent days, it is more probable that in this second case eight days only sufficed to develope the disease, and of these it is clear that the first four days were without signs of illness.

Guided by the law that a short limited exposure to the infection of measles is followed by a longer interval before the disease appears than when the exposure has been continuous, we infer that when the disease is of unknown origin, by looking back a fortnight the illness can be traced to its source, but that where there is free communication with those already ill, the malady will probably be developed in less than a fortnight, and not sooner than the eighth day from the commencement of the exposure. The next cases are confirmatory of these inferences.

CASES 51 and 52.—Two children, a girl and a boy, aged 6 years and 4 years respectively, who, with one exception, had certainly not met, or mixed with, or spoken to, any other children, are seized with measles on March 25th, and have the full rash of measles on the 26th; they had some catarrhal symptoms for two or three days, attributed to change of weather. On inquiry as to the events of the previous fortnight, it appeared that these children were taken to a friend's house in London to see the processional entry of the Duchess of Edinburgh on March 12th; a person had been ill with measles in this house a fortnight before, and two days after, some children resident in the house, who were in close communication with these two during the two or three hours of their stay, became seriously ill with this prevalent complaint. A half-sister, aged 18 years, comes home to the house where the two younger children are convalescent, on Wednesday, April 1st, associating freely with them during the day; she sickens, and is chilly, with loss of appetite on the evening of Wednesday the 8th; feels ill the next day; has some spots of measles on the 10th, but does not keep her bed till the 11th; the rash was at its height on the 12th.

The duration of personal infection after measles, is probably limited to three weeks from the time of the eruption. Infection is evidently as intense in the first week of convalescence as at any part of the illness, it is likely to be considerable in the second, and may persist into the third week. How completely this danger is over in the fourth week is shown by cases 24 and 25, where they not only mixed closely with young and susceptible children of their own family, but were also taken among others to a large establishment on the Day of Thanksgiving at St. Paul's, without being the cause of illness. The following case gives a further reason for believing infection to be over by the end of the third week.

CASE 54.—A boy, aged 9 years, son of a clergyman in the country; while at school, sickens for measles, on Sunday, December 7th, 1873. He remains away from home at Christmas, as his eight brothers and sisters, four older and four younger than himself, who have none of them had measles, are all at home for the holidays. On December 30th, exactly three weeks from the height of the rash, he returns to his family, mixing with the other children freely all day. He has a separate bed-room; his clothes were efficiently exposed to the fumes of burning sulphur, and he himself had a wash with carbolic soap; no one took infection from him.

This closely agrees with what is observed of small-pox, and contrasts markedly with what will afterwards be noticed of scarlatina.

After exposure to the infection of measles the signs of illness cannot be expected to appear in less than a week, nor will more than a fortnight elapse before symptoms sufficiently indicative of impending danger give notice that the suspected person must not yet be allowed to mix with the susceptible. Only in those cases complicated with hooping-cough, when the general symptoms were unusually severe,

did the kind of illness remain in doubt for more than fourteen days. In the one uncomplicated case that exceeded this limit, signs of the disease on the fourteenth day, sufficient to give warning of the impending eruption, were apparent. So that a fortnight's quarantine after exposure to measles will suffice to prevent unexpected outbreaks; in scarlet fever, even a shorter quarantine might suffice, for the rule for measles may almost be reversed in this case, and we may say, that if the disease does not show itself in the first week, or by the eighth day after exposure to infection, it probably will be escaped altogether.

Fifty of the cases of measles afford more or less accurate data for fixing the incubation period; the disease appeared in only eight days in one case, ten days in twelve cases, twelve days in seventeen cases, fourteen days in sixteen cases, fifteen days in two cases; once in sixteen, and once in eighteen days.

In thirty-eight of these cases the exposure to infection was either prolonged or indefinite, and the limits of the exposure undetermined. Of these cases one appeared in eight days, twelve in ten days, ten in twelve days, twelve in fourteen days, and three in from fifteen to eighteen days.

In twelve cases the period of incubation was accurately determined; of these five occupied twelve days, six occupied fourteen days, and one occupied sixteen days. This case, No. 17, was from a single exposure of short duration; the one case (No. 1) of shortest interval was from a continuous exposure, the earliest limit of which is fixed. No instance of nine days interval was met with. There may be a doubt as to some of the cases occurring on the eleventh or thirteenth day; the two cases (16 and 17), with the same exposure, occur on the fourteenth and sixteenth days, not on the fifteenth day. It is to be remarked that all the intervals of ten days only occur among the less precisely ascertained instances. This lends support to the conclusion previously stated, that the infection of measles is most commonly communicated while in the catarrhal stage only, and that a more intimate knowledge of the ingress in these cases would transfer them to swell the already preponderating balance in favour of according a twelve or fourteen-day incubation period to measles. In the essay of Dr. Gregory, one case only of measles is referred to, where, after a single exposure, the full period of incubation is fourteen days.

Only two clear cases (44 and 45) of second attacks of neasles have come under notice; most of the so-called cases

of second measles are attributable to epidemic roseola (*rötheln*, or *rubeola notha* of Babington), the rash of which, besides having some features distinct from measles, occurs without the precedent four days of catarrhal illness.

Rubeola, rosalia, or epidemic roseola, has an incubation period of from eight to ten or fourteen days, closely approximating therefore to true measles, morbilli, from which it is really distinct. That it is not second measles is well illustrated in the Cases 34, 35, and 36 of measles. At Christmas, 1869, these children, three girls, had not had measles, when two elder sisters, who had had measles in the previous year, have this rash, and communicate it to them; they have it in as characteristic a manner as the elder girls, but without seeming to be at all more ill with it. Two years afterwards those younger girls go through measles without any modification. Case 1: The eldest girl, a weekly boarder, where one of the school girls, usually much with her, had a rash, on November 6th, and was absent a week, from November 22nd, returns home on November 27th; next day she seemed ill, but woke up lively and well on the morning of the 29th, covered with this rose rash, finer, brighter in colour, less raised than measles, less diffused than scarlet fever, showing clear skin between the spots; she keeps her bed for two days, and gets up quite well on December 1st. Case 2: Her next sister sleeps in the same room, and lies near to her; she feels heavy and amiss on December 13th; by evening she has the rash, and goes to bed earlier than usual that evening, but not until she had spent two or three hours with two cousins who had come on an afternoon visit. Case 3: The elder of the cousins became ill on Dec. 24th (on which day she kept in bed) with rash on the chest and arms in the morning; by evening this extended to the legs, body, and face, where it was very conspicuous next day, though she herself felt pretty well again. Case 4: The younger, sleeping in the same room with the last case, felt sore throat on January 3rd, and had the rash on the 4th, just ten days after her sister was in bed with it.

To return to the first family: the youngest of those who had previously had measles, slept in the same room with the two elder sisters, but at a little distance from them; she, Case 5, became ill on December 22nd, and had the rash on the 23rd, also ten days after the second sister's illness. These girls mix freely during the day with the three younger sisters, who sleep in a different part of the house, and who have not had measles; each one of these, Cases 6-8, has

the rash early in January, or after another interval of ten or twelve days, without seeming to be at all more ill with it than those who had previously had measles. Three years afterwards these same children take measles, and the elder girls do not.

CASES 9 and 10.—A girl, aged 8 years, attending a day school, is very lively and well on June 5th, wakes up next morning covered with crimson spots; some remain sparsely distributed over the arms and legs on the 7th, but she is otherwise quite well. A younger sister, sleeping with her, complains of her throat on the 14th, but soon seems well again; they attend school together for a few days, and on the 22nd the second girl is noticed to have rose spots on her face in the afternoon; small, isolated, slightly raised, and not very bright spots occur on the chest and back at night. Next day the spots on the face form rose-coloured patches, not crescentic; isolated, slightly-raised pink spots occur on the arms, fewer on the legs, and very few on the feet.

These two cases are of interest as neither of these children had had measles. About three months afterwards an elder brother, aged thirteen, Case 11, who had measles some two years before, was sent home from school covered with a rash which, with apparent reason, was considered to be measles, for the eruption was very abundant, deep in colour, with very little clear skin between the spots: an elevation of temperature to 102°, and that disagreeable odour, on entering the room before ventilation had been seen to, which is so often noticeable in that disease. Neither of the sisters took any illness, though they were frequently in attendance on their brother, nor did three younger children who were kept more apart.

In a family of four children (two boys, aged nine and eight years, and two girls, of seven and five), the elder of the two girls, while absent from home, has rubeola; the others escape. Next year they all have measles, this girl equally with the others. Early in the following year the brothers return from a preparatory school with rubeola, and now the elder girl escapes, but the younger one takes it the week after the boys returned, and has sore throat and considerable hoarseness of voice.

CASES 16 to 21.—A boy at home for the Easter holidays, complains on April 2nd, 1869, of being tired, and next day has rash; he was secluded for two days, and then, on April 4th and afterwards, joined his family, seeing much of two elder sisters about his own age. On April 18th both those girls had the rash after complaining of giddiness and of feeling tired the night before the eruption. One of them had left home on the 15th, and communicated the complaint to two cousins who had previously had measles. A younger sister, not quite three years old, who associated less with the elder children, had well-marked rash on the 20th, with but little coryza and less-marked symptoms of illness than the others, though they had had measles, and she had not.

The time of incubation cannot be fixed in this series of cases, because the exposure was continuous. One of the girls leaves home ten days after the brother joins them. Had she been separated from that time, before visiting her cousins, she, in all probability, would not have carried infection to them, though less than a fortnight's quarantine would have been as uncertain against this disease as in preventing the communication of measles.

CASE 22.—A young man, W. F., aged 18 years, in business in the city (source of infection unknown), while training for athletics, becomes easily tired in the evening of May 6th; next day he is better, but has headache and sickness the day after, the 8th. On the 10th he faints during a foot race, but was able to resume his duties on the 12th, and continued at the office until the 17th. The rash appears on the 18th; he has a week's rest at home; he returns to his duties on the 26th. On the evening of the 31st one of the clerks in the same office complains of fatigue, and next day has the rash. Another clerk, quite well on June 3rd, has the rash on the 4th.

There may have been some common source of infection for these cases, otherwise the two clerks may either have taken infection from W. F. on his return to them, if the incubation period can be as little as from five to seven days, or, as seems more probable, they took infection from him on May 17th, before the appearance of the rash, with an incubation period of from fourteen to seventeen days. In the next case there is no such uncertainty.

CASE 24.—A sister by marriage, in the same house, but not much with W. F. till the latter part of the week that he remained at home, removes to another part of London the week after, May 31st, and has the rash on June 8th. She felt tired only the day before, but on the appearance of the eruption there was neither acceleration of pulse nor rise of temperature; nor was this the first time of her having a rash, believed to be identical, she having previously gone through both scarlet fever and measles.

CASES 25 to 30.—At a ladies' school of the highest class, with thirty boarders, a few girls from the neighbourhood are admitted to some of the classes; on Monday, March 30th, a day pupil was noticed about noon to have some spots on her face and went home; next day she was excused attendance because of a rash, nor did she return. On April 12th, one girl shows similar spots on the face; this was not thought much of until the next day, Monday, the 13th, exactly a fortnight after the day pupil left. Two other girls have also spots on the face, and the one first to show this has a decided eruption; two of these girls had slight headache with some tenderness of throat, and of some small glands on each side of the neck—one had no previous symptom. On each of the three following days one fresh case occurs, some having had similar sensations to the first two cases, also dating from Sunday the 12th; none of these had either coryza or cough; all these six girls had had measles from two to twelve years previously; in none was there any feeling of illness after the appearance of the rash. The longest period of incubation was here seventeen days. As some of the girls were sent home, a longer interval may possibly occur in other cases. Those who took the complaint were not specially in contact with the original case, and in only one could any predisponent variation of health be traced.

In half these cases of rubeola an incubation period of from ten to fourteen days is accurately determined: clearly as they all are distinguishable from second measles this similarity of development affords another sign of their near affinity. It is remarkable that, while only in our own day, by our first President, Dr. Babington, the separation of this exanthem from measles has been indicated; already the differentiation from scarlatina, by its long period of incubation, had been made by Dr. Maton in the year 1814, and published in the fifth volume of the *Transactions of the Royal College of Physicians.* Eight cases from one family are given; the latent period varied from seventeen to twenty-one days.

The two other diseases now to be noticed of the first group, varicella and mumps, correspond with measles in never appearing till after a week's interval; they probably require a longer quarantine, which, for the latter disease, must be extended to three weeks.

Varicella, or chicken pox, has an incubation period of rather less duration, subject to the same variations, as measles. The following ten cases give an interval of from ten to twelve days for incubation. This was prolonged to fifteen days in Case 5, where the infection was from a limited and not very certain source, and to sixteen days in one of the three supplementary cases, which were complicated with hooping-cough.

Case 1.—A girl, aged 7 years, who attends a day school, is noticed to have a spot on the lip on May 30th, and next day, other spots recognised as those of varicella; she remains at home, is febrile at night, with full eruption next day; the height of the disease was on June 1st and 2nd, when she remained in bed.

Case 2.—A younger brother of the above, occupying the same bedroom, is ill with chicken-pox on June 12th; ten days after his sister's illness was at its height.

Cases 3 and 4.—Two elder brothers of the above have the disease on June 13th. The interval in these cases was probably twelve days.

Case 5.—A girl, aged 3 years nearly, is visited by a nurse attending upon a child with chicken-pox on the last, or last day but one, of November. On December 14th this little girl has signs of chicken-pox, well marked on December 16th. A possible interval of sixteen days.

Case 6.—A boy, aged 7 years, brother of the above, at home with her, is taken ill on December 27th, with full eruption on the 28th.

Case 7.—A boy, aged 5 years, of the same family, absent during the illness of his brother and sister, is brought home to them on January 12th, because a child in the same room where he is staying has chicken-pox; he begins to be ill on the 21st, with the full eruption of chicken-pox on the 22nd and 23rd. Ten days' interval.

Case 8.—A boy, aged 7 years, returning home from a visit on January 6th, sickens for chicken-pox on April 13th, and is ill with it on the 14th and 15th.

Cases 9 and 10.—Two elder brothers return home from school on April

10th, and are much with their younger brother up to the time of his illness; both show slight signs of the disease on the 23rd; the younger of the two has high fever and full eruption on the 26th, and is some time before he recovers. The eldest, aged 16 years, who is known to have had this complaint in infancy, shows signs of rash on the same day, but the spots do not become vescicular; they are most numerous on the exposed parts of the body, namely the face and hands, appearing first around some recent scratches on the wrist; further rounded red spots, slightly raised, show themselves on the second day, but there is no fever, and the spots fade on the third and fourth days without any of them producing pustules or even vescicles.

VARICELLA WITH HOOPING-COUGH.—A boy, aged 5½ years, at a day school, where there is both hooping-cough and varicella, after a slight cough, barely exciting suspicion, stays at home on March 8th with spots of varicella, which are full on the 9th and 10th; finding on the 11th he is not very ill, he is sent to school in the following week; a week afterwards (a fortnight after the eruption), on the 25th, his cough again becomes frequent, and is very severe, with illness characteristic of hooping-cough on the 31st; this continues for three weeks, when the febrile disturbance terminated with the formation of pus in an old psoas abscess, the cough continuing to be spasmodic, but not very severe, for more than another month. *Vide* case 27 of measles.

CASE 12.—A younger brother, 2 years of age, has febrile disturbance attributable to the ingress of hooping-cough; during the week that the elder one is at home with varicella this febrile disturbance suddenly subsides on March 20th; there was a great diminution, both of gastric and pulmonary irritation on the 24th, and, on the 26th, the spots of varicella appeared in great numbers with the ordinary febrile disturbance, which however did not subside as quickly as usual, and, on April 1st, he had a temperature of 100° 4′, the crusted spots of the varicella still very numerous, and the hooping-cough more marked than before, accompanied with vomiting. Here the presence of hooping-cough had prolonged the incubation period of varicella to nearly its extreme limit, viz., fifteen or sixteen days; the varicella had intercepted the course of the hooping-cough for a week or more, and in all probability modified its subsequent course, for the child, though delicate, made a good recovery, and had less trouble from the cough than either of the others in this family. This boy is case 28 of measles.

An infant at the breast, a girl, aged 4 months, who was about to be vaccinated, had this operation delayed until March 31st, because of varicella, which appeared on March 22nd, ten days after the elder boy's illness. The day before the varicella showed itself, the mother feared that she had either taken cold or that hooping-cough might be threatening; a similar fear was expressed on the day before vaccination, but on that day there was no indication of illness sufficient to warrant a further delay. The vaccination took readily, the vesicles were mature on the eighth day, with areola on the ninth, when the progress was somewhat checked; previously it had been slightly accelerated, the febrile action of the seventh day being rather in excess of what is usually met with, and now a cough, noticed from the day after the vaccination, became suspiciously like hooping cough. The subsidence of temperature noticed in vaccinia upon the formation of the areola was not so marked as usual; on April 10th there was still redness of the arm; this was less on the 14th, and the febrile disturbance had subsided. There were no pulmonary *râles*, and the child seemed well, except for some fits of cough, with coryza; during this week the cough became very violent. On April 24th sickness followed the cough, and the act of sucking excited it; the crusts of vaccinia had not separated. The child became thin and ill, with more than one complication, to give rise to anxiety which was

not at an end until the middle of May, when a succession of small boils appeared under and near the vaccination marks.

The eldest boy in this family who had hooping-cough with measles (case 26 of measles), now escaped.

Mumps has fourteen days of incubation, which may be extended to three weeks; twenty-two days being the longest period that has come under my observation. The boy (aged 12 years, case 10 of measles), who must have contracted both measles and mumps at school before he left on March 20th, has signs of mumps on April 9th, exactly three weeks afterwards, and ten days after the rash of measles; he was removed on March 30th to a house where there were four children who had had measles but not mumps. They all four have mumps from the 23rd to the 25th of April, the shortest interval being fourteen days. Six cases in another family give an interval of from nineteen to twenty-four days. A boy, aged 8 years, attending a day school, who had not seemed very well for a week, has stiff neck on March 20th, 1870, and next day is febrile, with parotitis. His next younger brother and the nurse have mumps on April 8th; eighteen days. Two sisters sicken on April 9th and 10th; the mother on the 12th, and an infant (whose case is published)* on April 14th.

Four cases (13 to 16), in another family, are these: a boy is sent home from school unwell on March 11th, next day he is found to have mumps; on the 14th, a brother and sister, separated from him the day before, are sent to different houses, they remain free from symptoms till the 25th, when the girl complains of stiffness in the neck while eating her dinner, and next day has obvious mumps, thirteen days after the separation. The younger boy (case 14) has no disturbance of health until April 2nd, he was sent home on April 3rd, and next day, exactly three weeks from his leaving home, the parotids are swollen and the disease established. Three weeks after this a servant in the house (case 17) is taken with the disease on April 25th.

Where children are exposed to the infection of both mumps and measles at the same time, the measles, even if delayed in its development, will always precede the mumps, though some of the initiatory stages of mumps would seem to have been accomplished in the meanwhile, for it is only nine or ten days after measles that the mumps appear, instead of after a fortnight, which is the shortest period of incubation for mumps;

* *Temperature Observations.* J. and A. Churchill. London. 1871. P. 9.

or after three weeks, at which interval hooping-cough is generally found to follow measles. In hooping-cough following measles, it frequently happens that the measles only is communicated while at its height, (for there is reason to believe that in early convalescence from measles, or especially if measles have followed hooping-cough, that both may be communicated), so where mumps and measles have been taken together, it may happen that measles only was communicated as in the following instance (cases 18 and 19):—Two boys attended a day school last spring, where cases both of mumps and of measles had recently occurred; the elder of these boys has to remain away for a week from illness, sufficiently severe but of no marked type; the younger continues to attend the school till at the end of the week he has the catarrhal symptoms of measles; both boys have the rash of measles together, and a younger brother, who does not go to school, is allowed to remain in the same room with them, and indeed has been sleeping in the room with the elder all the week, the youngest boy is just beginning with the catarrhal stage of measles when the eldest shows signs of mumps, and two days afterwards the second boy has mumps; the youngest was removed to another room as soon as he became ill, and did not afterwards take mumps.

CASE 20.—A lad, aged 15 years, left school on December 17th (one of his schoolfellows with whom he associated began with mumps the day before); he has no feeling of illness until December 24th, when he was restless at night, next morning he vomited, and then felt better; on the 26th he was able to skate though easily tired; he came to London on December 28th on a visit, and was noticed not to be looking well; he felt and looked better on the 30th, except for some swelling under the right ear; there was increased swelling with pain and illness, next day, December 31st. The submaxillaries were swollen on January 5th, the left parotid on the 7th, and he was well again, except for almost imperceptible swelling of the parotids, on the 12th. For the latent period and the stage of invasion, each a week.

Six schoolfellows who left school on the same day had mumps about the same time—all early in the new year—between a fortnight and three weeks after exposure. In this case the disease cannot be said to have begun until the fourteenth day after leaving, viz., on the 30th of December; the sickness on the 25th probably marks the commencement of the invasion period, occupying exactly six days, thus leaving eight days for the period of latency which other cases show may in this complaint be easily extended. Infection can hardly be said to have begun in this case until the first local signs (which in other instances are shown to follow very quickly upon the first progressive elevation of

temperature) were perceptible. Five children were at home with him from December 17th to 28th, none of whom took the disease; he returned home also after a fortnight's quarantine, and did not communicate it to others. Case 14 shows that for preventive quarantine, where it is doubtful whether the poison of mumps has been received, fully three weeks must be allowed; here, again, a short limited exposure with complete separation does not, any more than in measles, serve to mark the shortest incubation period, but may, in fact, be followed by a longer interval of freedom from all signs of the disease than when the exposure has been more complete and continuous.

The close parallelism between the ingression of the diseases hitherto considered, is worth notice; an initial fever generally marks the invasion period; this, in measles, is often to a considerable degree, sometimes nearly equal, but never quite, to what takes place at the acme just before the eruption; at others it is either less obvious, or attention is withdrawn from this part of the illness by the temporary depression of temperature that follows it. This, however, has but little practical importance, for it is not until progressive febrile disturbance, easily recognised by the thermometer, marks the real beginning of the disease that infection begins. This usually is four days before the eruptions of small-pox and of measles (probably not so long before mumps), and only one, or at most two, days before the specific indications of chicken-pox, and rubeola.

Hooping-cough may be considered at the end of this group, though many of its affinities are with disease of the other class. Facts are wanting to show what is the period of incubation; this stage being frequently prolonged or overlooked, is one great cause of the extensive spread of this disease, for it is quite as infectious in the early catarrhal stage as when the hoop is developed.

That the incubation period of hooping-cough may be a short one is shown by a case previously published,* where a girl, aged 12 years, is brought home to a family with hooping-cough on July 25th, has spasmodic cough on the 30th, with bronchial râles and the hoop on August 2nd, the eighth day after; it is rare to meet with the hoop until the second week, but not, I believe, to find evidence of the disease within a week of exposure. Where separation is effected, more than a week must be allowed for quarantine purposes;

* *Infantile Temperatures.* J. and A. Churchill. London. 1869. P. 26.

this is illustrated by the following case which also shows that hooping cough is infectious in its earliest stages before the hoop is developed. The younger children of a family in the north-west of London associate with another family in the Square Gardens, who, after an absence from home, return from the seaside at the end of August with hooping-cough; after September 2nd there is no further communication between the families, but during the week one child, a boy, aged 6 years, has a cough, which by September 7th becomes frequent. An elder sister, aged 15 years, who is a good deal with this boy, but had not been in the Square Gardens, is sent from home to escape the cough on September 9th. During the next few days all the younger children begin to cough, but letters from the elder state that she has escaped and is quite well; this is considered to be most fortunate, as the aunt at whose house she is visiting has an only daughter, 8 years old, who has always been considered delicate, and who is watched with care. On September 13th the elder girl first makes mention of a cough, she afterwards describes this as "fidgetting," and complains that she could not sleep at night. On the 19th she is seen by the doctor, who is unable to recognise any of the distinctive signs of hooping-cough; next day and the day after more violent paroxysms of cough leave him in no doubt as to its nature, though she felt better herself and was able to sleep when the paroxysm of cough was over. The cousin, who had never occupied the same room at night, was now kept more apart during the day, but it was too late; a short irritative cough commenced, and by the end of the week she becomes languid, and is soon prostrate and febrile. The character of the cough was quite pronounced by September 30th, and was afterwards very severe. A serious illness continued till October 16th.

The infection of hooping-cough can be conveyed: a stout boy, 2 years old, quite well except for cutting the two last molars, is visited by some friends on November 20th, who have a child at home with hooping cough; this boy coughs on November 24th, and is seriously ill with lobular pneumonia the last three days of November and the first two of December. On December 8th there is no pulmonary mischief, but he has short cough, worse at night, with thin loose secretion, and hoops on December 15th.

Cases of hooping-cough published at pp. 40 to 52 of my "Temperature Observations in the Diseases of Children,"*

* J. and A. Churchill. London. 1871.

and others that need not now be detailed, give generally a week for incubation, and a week or rather more for the febrile or catarrhal stage, so that the hoop is seldom developed until after a fortnight from exposure. In the course of hooping-cough an intercurrent inflammation often stops the hoop, a fact impressed into the service of various theories; such accidents certainly interrupt and probably delay the ordinary course of the disease; what often happens in the more advanced stages of the disease sometimes occurs at the commencement, as in the last case given, when pneumonia occupying five days of the ingress, the hoop instead of appearing on the first week of December, was delayed till the 20th, and this is frequently found to be the case from much slighter causes. If such prolonged intervals are to be considered as "incubation period" then is hooping-cough closely approximated to the diseases previously treated of, and it may be concluded for all lessons of practical precaution, as in those cases, so here, that if a child has been exposed to infection, a fortnight may elapse before the symptoms of illness are declared, nor can such child be considered free from the chance of conveying infection until after three weeks. On the other hand, while it is probable that small-pox could hardly be communicated for the first eight or ten days from exposure, or mumps for nearly a fortnight, hooping-cough has the dangerous prerogative of being communicable from the earliest symptoms that may appear within three days of infection, as well as by the later manifestation of the disease, that may be delayed if not indefinitely, at least for two, and perhaps even for three months.

The poison of hooping-cough, like that of scarlet-fever and others, may be intensified both by the re-infection of those in certain states of weak health, or by being concentrated where several sufferers are congregated or kept too much confined either in small rooms or else in the same room. A case is given at p. 43 of my *Temperature Observations*, where a child was in considerable danger until removed from the room where another was ill.

Fatal consequences resulted in the following case from this cause, further intensified by allowing the dead child to remain in the dwelling:—A girl, 2 years old, while teething, has a cough, at first attributed to that cause, but suspected to be hooping-cough on February 2nd, and recognised as such on the 10th of February, when a brother, 4½ years old, occupying the same room, has a similar cough. On this day the mother gives birth to an infant, who, thriving very well

for the first week, after that is noticed to pine and have difficulty in taking the breast—there is no doubt of the infant having hooping-cough, on the 24th of February, it was sick with its cough, and before the end of another week could hardly suck, as the first touch of fluid in the fauces induced violent spasm. On March 1st the infant dies; at this time the little girl's cough was improving greatly, and though the boy's cough was violent he was otherwise doing well, and had a strong well-formed chest. Instead of the infant receiving prompt burial it remained in the room, there were but two on one floor occupied by the family, until March 8th, when, in spite of chlorine and other disinfectants, an offensive atmosphere was induced. On March 10th the boy's cough had increased, the fauces became red and irritable, there was viscid phlegm in the mouth and difficulty of taking food; on the 17th he had dysuria, with marked pyrexia, afterwards convulsions, and death on the 24th of March. The girl's cough was also increased at the same time, but her health improved by the end of the month, and after April 2nd her convalescence progressed rapidly.

A frequent mode by which the poison of hooping-cough spreads, is by persons who have previously had the complaint, while much with the sick, suffering a second attack in a modified form, which, by not exciting alarm, leads to neglect of the requisite precautions against conveying infection. Fomites readily attach themselves to surfaces, whether articles used by the sick or near them, and to the clothes of attendants; these infecting particles, probably minute solids, cannot be wafted far, they may be carried by the healthy, but the most frequent mode of diffusion is by liberation from the breath of the sick.

Scarlet-fever is the typical example of my second class of infectious diseases to which these latter properties, that are also attributable to hooping-cough, belong. The incubation period is essentially short, and may be very short, even to a few hours, the more usual period is from three to five days. A necessary question arises as to how long may this period be extended; the longest interval I have met with has been eight days, and when children have been removed from a source of infection if any have taken it they will most probably sicken within the week. This also is true of diphtheria, of which several instances have come to my notice.

Scarlet fever is not so infectious during the first day or two of sore-throat as it afterwards becomes, so that an early

separation gives good hopes of immunity, especially if the exposure has not been at very close quarters, such as sleeping in the same bed; even a moderate distance, about two yards, between children's beds, is some safeguard. Persons suffering the sore-throat, so often experienced by those who, having previously gone through the disease, are attending upon scarlet fever, are not free from the fear of infecting others: an instance has lately come under notice where a nurse had such a sore-throat, with aching of limbs, and only a slight degree of fever, enough, however, to oblige her to go home for two or three days; her husband, who had never had scarlet fever, takes the complaint with great severity.

The way in which the infection of scarlet fever clings to surfaces and rooms is well known, it may thus lie dormant, as in the case of small-pox, for months, or possibly for years. By these means the disease may unexpectedly make its appearance, when by some chance a debilitated or susceptible person is confined to the infected chamber, or exposed to slight traces of infection that may linger on clothing. The latter cause is illustrated by two cases occurring on board ship, published in the *Lancet*, September 3rd, 1864, p. 279, by Dr. W. M. Saunders, R.N. A naval cadet of H.M.S. *North Star*, has symptoms of scarlet fever on February 21st, 1842; the ship left Madeira, the last land touched, on 1st of January, where the lad visited at a house wherein some children had recently had scarlatina. "The clothes which he wore at Madeira were most probably worn on each Sunday during the voyage."

A midshipman of H.M.S. *Shannon* was put on the sick list 2nd May, 1857, for a wound of the foot; on the 7th of May he was attacked with febrile symptoms which proved to be those of scarlatina. The ship anchored in Simons Bay, Cape of Good Hope, on the same day, not having communicated with the shore, or with any vessel since leaving England, on the 25th of February. The *Shannon* was at Portsmouth, in February, 1857, when some children near this young man's father's house at Southsea, had scarlatina. Here, although the infection had lain dormant for nine weeks, it becomes operative in the usual period of incubation, five days, after a susceptibility is induced by the wound.

An instructive instance of continuous exposure to the poison of scarlet fever becoming dangerous only upon the receipt of an injury is given by Mr. Henry Veasey, of Woburn, Bedfordshire, in the *British Medical Journal*,

1869, vol. i, p. 113. A servant, aged 30, in a family where scarlatina had prevailed two months previously, with desquamating convalescents in the house, but no new cases of fever for the last month, cuts the knuckle of her right middle finger against a child's broken mug, on May 27th. The wound was contused and painful, it was the girl's menstrual period. She is sent home the same evening. Next day she has rigors and erysipelatous swelling of the hand. Her illness was extreme for some days. On the fifth day, May 31st, there was sloughing at the seat of injury, vesications in the right arm, rash on the neck, chest, and left arm, with sore throat, full rash, and all the characters of scarlet fever well marked by evening. The patient recovered; a nurse in attendance upon her at the height of the illness, went home at night, and on June 3rd, the nurse's child, aged 3 years, had sore throat, and scarlatina rash well marked on the three subsequent days.

It is not likely that the poison of scarlatina was introduced only by the wound in the hand; doubtless, material had already been received sufficient to set up the disease, when the accident gave it a starting point. The first evidence of small-pox, measles, and varicella will appear at, or near a recent wound or abrasion of the skin, when the special infection must have been received days before the injury. Sir James Paget, in a clinical lecture, gives instances of scarlet fever following upon surgical operations; most of these are from the nurse having introduced the infection at the time of the operation, or soon after, the disease appearing at a time closely corresponding to the ordinary incubation period of scarlet fever. In one case of lithotomy in a boy, followed first by scarlet fever, soon after that by hooping cough, this probably taken during convalescence, there is reason to believe that the poison of scarlet fever had already been received before the operation; the first signs of scarlet fever gave way to suppuration from the wound, after that had ceased the scarlet fever symptoms returned and went through their full course after a fortnight's interruption; how long such an interruption and delay in the development of scarlatina may be prolonged is uncertain; ten or twelve days would be a short time to allow for such a possible extension of the incubation period, and consequent quarantine limit. A still longer time would seem possible in the case of typhoid fever as shown by the instance presently to be cited from the experience of my friend Mr. Adams. It may be that the complications of

intercurrent disease, by interrupting the morbid process of scarlet fever in their midst, tend to prolong the subsequent duration of infection as in this case:

CASE 1.—A girl, aged 9 years, probably exposed to scarlet fever on January 13th, has the first symptoms on the 14th; on the next two days there is delirium and a full eruption, this subsides on the 19th, when articular rheumatism began. Pericarditis, with effusion, existed as a serious complication from January 24th to February 10th; there was then some otitis and paralysis of the palate, and afterwards of the abdominal muscles. Convalescence having fairly set in, after a month's quarantine, she is sent into the country, on March 20th, to join other children of the family, nine weeks and a half, or sixty-six days after the commencement of her illness. A younger brother meets her at the railway station, and, in returning home, sits opposite to her in a close carriage. On March 22nd this boy is ill, he vomits, and is very restless at night; next day has sore-throat and rash, with full rash on the 24th and 25th; the illness is severe, and continues to the end of April. Two children, one older and one younger than this one, escape.

Another instance of the length of time scarlet fever continues to be infectious, is the following, in which there is no important complication to draw attention from the fact that personal infection may linger, not only far beyond the time of desquamation, but after the clearing away of the last visible morbid product of the disease.

CASE 2.—A girl, 10 years old, comes from school on July 4th with scarlet fever; the attack was severe, but uncomplicated. Throughout August one nostril is tender and obstructed, the tonsil on the same side is full; there is eczema of that ear. On August 14th, six weeks after the commencement of the illness, this girl is sent to the sea-side, where she joins her two younger brothers; the youngest, aged 5 years, has sore-throat on August 16th and the full rash of scarlet fever on the 20th.

CASE 3.—The other brother, aged 9 years nearly, is separated from the younger, and returns to London with the convalescent sister, who has now no vestige of the illness, on September 4th; they stay at the house of a friend, who has one grown-up daughter, in whose room the girl sleeps, from the 4th to the 10th of September; the boy goes to his own house on September 6th, he has slight sore-throat on that day, is feverish on the 9th, and full rash on the 10th.

CASE 4.—The young lady with whom the girl was staying, is taken suddenly ill on the afternoon of September 10th, with headache, lassitude, and chills; next day she vomits, and has sore throat, with intense aching of limbs and prostration; there is the full rash of scarlet fever on the 12th. These attacks either originated from the girl, again nine weeks and a half, or sixty-six days from the commencement of her illness; or the boy communicated scarlet fever four days before he is himself ill; he probably contracted the disease before leaving the infected house, as the symptoms appeared within three days of leaving it.

To give all the instances of short incubation in scarlet fever would be to exhaust a record of cases more numerous than those of measles. It sometimes happens that of children with equal exposure to either infection one escapes. In measles there is danger of attack while the children re-

main together, even after a month or more has elapsed, but if they are separated, and the room and clothes disinfected, the danger of a fresh outbreak is little or none; this is not the case with scarlet fever, when local causes of disease may remain after every means of disinfection have been practised.

CASE 5.—Of two young children, one, a girl, has an illness in June, which, after some days of uncertainty, is recognised as scarlet fever; it is considered too late to separate them, and on the appearance of some slightly suspicious symptoms, the other, a boy, is placed in the same bed with the girl; he was soon better again, continued much with his sister throughout the illness and during convalescence; they both make a long visit to the sea-side, and return to their house early in October; within three days of their arrival the boy is taken ill.

Every care had been taken to disinfect the house, and special care as to the room and everything in it; the child's bed had been near the fire-place, and at the foot of a mahogany polished bedstead, with rounded end, on washing this surface with diluted Condy's fluid, the end nearest the child's bed discoloured several basinfuls; the corresponding round end of the footboard furthest from the child's bed caused hardly any discolouration; some shells used as playthings by the sick child also required repeated washings, others exactly similar not used by the child did not change the weak solution of permanganate of potash used. The local cause of infection here was caused by a neighbour having appropriated the rain water pipe, having an opening near the child's bedroom window, into a soil pipe, for a water-closet.

In another case of doubtful scarlatina occurring to a little boy, (case 6) during hot weather, on July 10th, commencing with diarrhœa, on the 7th the symptoms had so rapidly disappeared that he was allowed to mix with other children on July 16th; on the 19th two other children were ill with scarlet fever.

CASES 7 and 8.—Of two little girls living in an isolated, perfectly healthy house, the elder, 4 years of age, has a rash on February 21st, thought to be measles, and attributed to contagion from a child met with on February 17th, when she was taken to a London hospital, this being the only occasion for some time on which any communication with others could be traced. On the evening of the second day after this, February 19th, the child vomited; on the 20th she had sore-throat; the rash now appearing was evidently that of scarlet fever. The younger child, aged 2 years, was also ailing on the 21st, and seemingly had some difficulty in swallowing on the previous evening before there was any appearance of rash on the elder child. Both had rash on February 22nd, that on the eldest being most diffused and intense; next day the elder has albuminuria. They die, poisoned by scarlatina, the younger on February 26th, the elder on March 2nd, leaving their parents child-

less. The younger child took the complaint from her sister, if not while she was only sickening for it on February 19th and 20th, yet certainly before the 21st, when the rash appeared.

Terrible consequences not unfrequently result from allowing children from a house or family, infected with either scarlet fever or diphtheria to go direct to school; beyond the chance of infection being carried, is the certainty that if such child had only the first process of either disease set up in themselves, they will diffuse it widely before it is known to have commenced; quite recently a widely spread outbreak of diphtheria originated in this way: a scholar continued to attend school while a member of the family at home was ill with diphtheria, no harm resulted until soon after the first symptoms of illness were felt by this scholar, and though further attendance at the school then ceased, nineteen children in the school were attacked. The closest correspondence is found to exist between the laws of infection in scarlet fever and diphtheria, as is fully set forth in the article on the latter disease, in the first volume of Reynolds' *System of Medicine.*

Influenza is more closely connected with scarlet fever and diphtheria than is commonly known. Prevailing under two forms, the catarrhal and the herpetic, it has an incubation period closely corresponding to what is observed in those diseases; this may either be very short, or may occupy three or four days, but will not extend beyond a week. The shorter period generally obtains in the catarrhal form, the longer in the herpetic. The two forms, though most frequently propagating their own specific characters, are capable the one of giving rise to the other; instances of this are given in my *Temperature Observations*, p. 33; perhaps the catarrhal form more frequently gives rise to the herpetic, but they are really interchangeable. It would seem that the poison in a diffused state, acting on a healthy person under healthy conditions, may be rapidly thrown off with catarrhal secretion, while the same poison if concentrated and acting in different conditions of health, or under insanitary surroundings, may show itself less quickly but with more profound effect. Hence the affinities of herpetic catarrh with croup, ulcerated sore-throat, and diphtheria; those of influenza when mild, with common cold and diarrhœa. What in the winter is cough, in the summer affects the gastric mucous membrane rather than the pulmonary, resulting in diarrhœa; this may go through a household, as does catarrh: one person visiting another

with summer diarrhœa, and staying in the sick room, is liable shortly to be similarly affected. The epidemic form of influenza has some affinity with scarlet fever, both in the rapid action and in the intensity of the contagion; this, though acting on different tissues, and productive of less serious lesions, is much more readily spread by atmospheric, or other less appreciable modes of conveyance.

Typhoid or enteric fever, with an incubation period generally of ten or twelve days, which may be considerably prolonged, may also be developed in so short a time as four days. Dr. Clifford Allbutt records* the exceptionally short interval of five days in a child after sleeping one night with a nurse ill of typhoid; the evidence if not quite conclusive in this case is presumptive of the possibility of so short an incubation period. One case of typhoid from tainted milk which came under my notice, occurred on the fifth day from that on which alone the poison was imbibed. Dr. Murchison† gives a case from Knævenagel's work on the Etiology of Typhoid, of a man who falling into a latrine has typhoid fever eight days afterwards, some of the poison probably being received by inhalation at the time of the accident.

An instance of definite and short incubation of typhoid from inhalation has recently come under notice in a house near Grosvenor-square, most carefully guarded against the entrance of sewer gas, and the usual sources of water contamination, the pipe drains being well ventilated, by air tubes from below the trap of each water-closet to the roof. A change in the main sewer obliged the house drain to be carried from the back to the front of the house; for some days in May, workmen were employed in putting new pipe drains in the previously undisturbed basement; from this work no mischief could arise until the old communication with the outer drains was severed, or the new one opened. From the memorandum of the work done, supplied to me by the contractor, the period of danger appears to have been limited to three hours, on Saturday, the 24th of May. No defect was noticed in the portion of the drain removed, unless there had been slight leakage from the scullery sink at the back of the house, this was remedied on Friday, the 23rd, and lime was added to the earth surrounding the new

* *British Medical Journal*, 1870, vol. i, p. 480.

† *A Treatise on the Continued Fevers of Great Britain*, by Charles Murchison, M.D., LL D., F.R.S. Second Edition. Longman, Green & Co., London, 1873, p. 469.

drain. The opening in the basement of the house where the old drain passed through was closed with cement.

The following is the workmen's account:—Thursday, 22nd May, putting waste to sink and preparing soil pipe; Friday, 23rd May, repairing new soil pipe, and putting in drain pipe; Saturday, 24th May, assisting putting in drains, opening front vault, shore drain for syphon, connecting servants' water-closet and soil pipe, opening old drains and making air tight, 3 hours; Monday, 26th May, making good paving and soldering in air-pipe.

Beyond a somewhat sickly smell under the sink, removed by the lime, nothing disagreeable was noticeable until during the three hours on Saturday, when one of the servants, a delicate woman of 30, complained more of it than the others, and consequently went out in the evening for fresh air, and to see the illuminations on the Queen's birthday. She made no further complaint, nor did she feel any illness until seven days after, when, on Friday, the 30th of May, she again felt nausea, and returned from a walk tired, with a fixed pain in neck, and diarrhœa; at night she was sick, the diarrhœa continued; she complained of chills, headache, night fever, and restlessness, with occasional vomiting, and persistent diarrhœa. On June 5th her evening temperature was 104.3, with nearly the same elevation for the next five days, the morning temperature ranging from 101° to 101°.8; during this time very characteristic rose spots appeared. By June 15th, the diarrhœa and febrile disturbance were moderated; there was slight nocturnal delirium and great prostration. In the last week of the illness, bronchial râles, with free expectoration, a tendency to constipation or inactivity of the bowels, and considerable emaciation; very little solid nourishment could be taken till June 30th, when she first left her bed. Some desquamation was noticed both of the feet and arms, especially near the elbow, where the skin was red and thin. No one else suffered in the house. The milk supply was unexceptionable. This servant had no friends in London, had visited no other house, nor taken either food or drink elsewhere.

The most frequent source of typhoid is the diffusion of the poison in drinking water, whether contaminated by direct admixture of sewage, or tainted by sewer gas. The outbreak at Guildford, investigated by Dr. Buchanan,* affords definite data for estimating the ordinary incu-

* *Tenth Report of the Medical Officer of the Privy Council*, 1867, p. 34.

bation period of typhoid. A new well for the supply of water by steam power to the higher parts of the town of Guildford was constructed in fissured chalk, in such proximity to the old sewer that contaminated water was pumped into the reservoirs until August the 1st; from that date to August the 17th, the high service drains were charged from the old well, when from some interruption to this source of supply, the water stored before August 1st in the new high service reservoirs was distributed to 330 houses. "It was distributed on no other day, and to no other houses, and on these 330 houses the fever almost exclusively fell." The first cases happened on the last three days of August; houses that had no cisterns, but took their supply direct from the mains, were attacked particularly early and suddenly; many houses had cisterns, or large underground tanks, in which the water received on August the 17th would be stored. Other cases of typhoid fever came under observation in the first two days in September, and on September the 3rd and 4th, a surprisingly large number of people sent for medical assistance, and were found to be suffering from the same fever. During the month, 264 cases of fever were under treatment. Dr. Buchanan fixes the occurrence of the first symptoms, in a great proportion of the cases, eleven days after the operation of the cause. A fact of interest in connection with the transitory operation of the cause is the low death rate, only three cases died in September. The total number of cases was 500, of the deaths 21; the last death was on November 22nd.

Much uncertainty as to the time necessary for the development of typhoid, is owing to the difficulty of ascertaining how long, or how continuously its course has been operative. Some persons exposed to it would escape with slight symptoms of malaise, did not a continuance or a repetition of the exposure at length overcome their powers of resistance, and they fall victims to the disease, not from the effects of the last only of the exciting causes; yet if this alone were taken into consideration the period of incubation would be unduly shortened. Others, after, or during exposure, suffer from diarrhœa, and may escape with no other symptoms, unless by sudden interference with the accustomed eliminative effort, or some other change of condition the disease is set up; hence the incubation period may be unusually prolonged. Mr. Adams, of Harrington-square, was attending a gentleman for troublesome diarrhœa persisting for more than a fortnight after his return from a continental town of

bad sanitary odour; the diarrhœa aggravated an ulcer or fissure of the rectum, and the pain was so intense that the usual operation was necessitated; while the incision was suppurating freely, no diarrhœa and no discomfort occurred, but on the complete healing of the fissure a sudden rigor ushered in a rise of temperature with ensuing exacerbations, ochrey diarrhœa began, there were rose spots in the second week, and all the symptoms of typhoid continued their usual course.

A convalescent patient probably does not give off infection long after the symptoms cease; but for how long typhoid might in this way be carried to distant parts is conjectural; that it may become active long after it has found its way into water sources is well known. The poison of typhoid, like that of cholera, acquires increased activity after it is thrown off from the human body, under the co-operation of heat and moisture upon filth; contaminated water is the great medium of conveyance for those poisons, whether directly or by subtle particles of them diffused into the air, and condensed into water surfaces, or carried by sewer gas directly into drinking water. Personal infection, rare in both instances, is rarer in typhoid than in any other of the diseases under consideration, yet the infection, however propagated, derives its origin from persons previously affected. Certain forms of illness of irregular duration, generally shorter than that of typhoid, and often characterised by diarrhœa, are probably owing to the poison of typhoid, and may probably reproduce it: such cases are given at pp. 55-60 of my *Temperature Observations*, and Professor Jürgensen, of Tübingen, has recently called attention to numerous cases of this kind.

Typhus has been so exhaustively examined in the masterly essays of Dr. Murchison, that it becomes merely necessary to refer to the second volume of the *St. Thomas's Hospital Reports* for 1871, new series. The incubation period is twelve days; of thirty-one cases there collected seventeen occurred at this limit; one case in not less than thirteen days; one in fourteen days; one exactly fifteen days; and in one it was not less than twenty-one days. Ten cases, or nearly one-third of the whole number, fell below the limit of twelve days; of these, in two it was not more than ten days; in one not more than six days; in one exactly five days; in one between five and a-half and six days; in two not more than four days; in one not more than two days; and in two there was no latent period, or only one of a few hours. In this disease it is to be remembered that the distinctive rash does not

appear till the fourth or fifth day of illness. The remarkable way in which the infection of this most infectious of diseases is diluted and soon destroyed by free ventilation and fresh air, is forcibly stated in the great work on fevers already referred to; one statement there, however, requires correction: instead of convalescents from typhus not having the power of spreading the disease, a medical friend of my own, impatient of precaution during rapid convalescence, hastened from London to a distant part of the country, and communicated typhus in two remote villages, where nothing of the kind had previously been known.

Relapsing fever is conspicuous for the shortness of its incubation period. In numerous instances persons are attacked at once upon exposure to sources of strong infection. In the Camberwell workhouse, tramps received at night to infected quarters, were found next day to be suffering from the disease. The cases related by Dr. Marsh,* where the febrile rigor succeeded immediately to the application of the contagious effluvium, were most likely of this kind. Very clear instances of a five days incubation period have been recorded of this disease, and it has been stated to be oftener over than under five days. Of twelve accurately determined cases, three were immediate; two from two to four days; two were five days; and one each in seven, nine, twelve, thirteen, and fourteen days. The possibility of latency to the fourteenth day has, however, yet to be determined.

Yellow fever usually with a short incubation period of from two to five days, and rarely exceeding eight, has yet been stated sometimes to extend to ten days. In the thirty-eight cases reported by Inspector-General Lawson on board the *Bristol*, some began to appear two days after exposure; twenty-nine of them occurred in the first eight days; the one exceptional case was a week after the others, thus suggesting the possibility of reinfection. This case falling fourteen days after the original exposure, cannot be used to justify a quarantine of a fortnight's duration, for the possibility of this length of incubation period is not beyond doubt; if, after leaving an infected port, no case of disease have occurred by the fourth day, that is sufficient evidence that neither crew nor passengers had contracted the disease before they left, and that further quarantine is unnecessary.

Plague, now nearly extinct in Europe, is fortunately characterised by a short period of incubation. Dr. Gregory

* *Dublin Hospital Reports*, vol. iv, 1827, p. 455, case 5; see also pp. 518-20.

writes "From the concurrent testimony of numerous authors, we are warranted in saying that the period of incubation of the true pestilential germ is very short." "Two days have been stated as the minimum, and fifteen as the maximum; five days may be looked upon as a fair average. Four days suffice to set up the constitutional symptoms when the plague is received by inoculation." The shortness of the incubation period offers an additional character to those noticed by Dr. Tholozan as distinguishing plague from typhus.

Dengue, an eruptive disease, is, like influenza, a pure epidemic, first attacking a few and extending its circle till few escape; the ingress is sudden, with febrile and articular pains, followed by a scarlet rash; it passes off without danger, but leaves debility, a tendency to relapse, with pain and weakness of the joints. The stage of incubation is a short one, but may extend to three or four days; one attack is not protective against another. Infection will cling to surfaces, and may linger long on board ship, breaking out unexpectedly; in 1870 this happened among troops conveyed by sea from Bombay (where it had extended from Zanzibar) to Cannamore on the Malabar coast; in 1871 the epidemic extended to Madras and thence to Calcutta, Rangoon, and Singapore; in 1872 it had extended both to China and to the Punjab. Dr. Taylor* notes the disease first in Delhi on June 1st, 1872, attacking one member in an European family that had arrived from Calcutta three days before; subsequent seizures were observed amongst persons who had been in communication with this family. The disease prevailed as an epidemic from July to November. Almost every one exposed to the contagion caught the disease; when once it entered a house hardly a person escaped. One fatal case only is reported in the whole course of the epidemic. Common in India, where scarlet fever is rare, dengue would seem to be of African origin, and to owe its extension to the slave trade. It has not yet appeared in Europe. In the West Indies it was first described as a hybrid between urticaria and rubeola, as with us rubeola is sometimes spoken of as scarlet fever and measles conjoined, a confusion of totally different morbid conditions, possible only so long as the distinctive characters of a disease remain unrecognised.

* *Report of the Sanitary Administration of the Punjab for the year* 1872, p. 70. Other particulars of this exanthem are from the address of our President, Dr. W. R. E. Smart, C.B., Inspector-General of Hospitals, Royal Navy.

Cholera, whatever may be said of its mode of propagation, is distinguished by the shortness of its incubation stage; hence the readiness with which quarantine measures may be made effective in a country such as our own. From inquiries made by the Central Board of Health in 1831, of 171 cases at Berlin 159 were found to have occurred within five days from exposure to the infective germ. At St. Petersburgh, in the cases where single exposures to infection were best ascertained, the period of incubation ranged between one and five days. In the Austrian territory, the Genoese Medical Commission observes "that those who had absorbed the germs of the disease were generally attacked before the third, and not later than the fourth day." The difficulty experienced in the early recognition of some diseases is no obstacle to the efficiency of preventive measures against cholera. The premonitory diarrhœa generally affords an obvious symptom, and it is well known that the infection at this stage must be guarded against. One attack of cholera affords very slight, if any protection, against recurrence.

Until quite recently typhus has been often confounded in India with remittent fevers; ten times the number of deaths in British India are registered under the head of "fevers" to those from cholera during the worst epidemics.* In the report already referred to, Dr. Fairweather states, "In no instance do medical officers attribute these severe forms of fever to any but a malarious origin. The manner of distribution seemed to point more to a *specific cause* than to any general condition. In the early part of 1872 typhus fever was found mixed up with the ordinary fever of the country in a jail in the north-west of India;† it was known to have prevailed here before, but had escaped detection. How much more difficult must it be to detect the presence of a specific fever among the mass of the native population. That fevers peculiar to this country do exist few will be prepared to deny. No mere general descriptions will ever be useful in determining the nature of a disease, and it is

* *Ninth Annual Report of the Sanitary Commissioners with the Government of India*, p. 207.

† *Report on the Sanitary Administration of the Punjab for the year* 1872, p. 68. Dr. De Renzy directed an inquiry on this point; the careful clinical observations of Assistant-Surgeon Lombard proved the fever in question to be typhus; he caught it and died. The temperature observations he made leave no doubt as to the nature of the fever of which he himself fell a victim; yet on these very proofs, with the usual evening exacerbation, the local medical authority remarks, "The disease therefore was, as far as the temperature is concerned, decidedly remittent."—Appendix to Report.

only by an officer who can make minute clinical notes of cases, such as were made by Dr. Lombard, that the tangled skein of Indian fevers will ever be unravelled." No long time has passed since among ourselves typhus was called endemic, under the name of jail fever.

The London bills of mortality included many cases of typhoid or enteric fever under the name of ague; the death of James I is a probable instance. How long a new resident in a malarious district may escape, or how soon he may suffer, was seen in this country, when our old ague grounds were undrained; Irish harvest men, a month in the fens, were sometimes attacked before their work was over, many were seized on their journey home, others long after their return. Malaria, short of inducing ague, produces some ill effects not immediately noticeable, but developed on some subsequent exposure to any cause of illness, even though this be experienced long after quitting the malarious district.

Malarial fevers, having many points of resemblance to epidemics like yellow fever and cholera, though non-contagious, are found also to have an incubation period, which, though capable of being accelerated or retarded, is yet as constant as that found to obtain in so specialised a disease as typhoid. Dr. Walter Dickson, one of our secretaries for the Navy, informs me that the non-contagious malarious fevers of the west coast of Africa often show an incubation period of twelve days. At about that interval, after the return of a boat's crew from a river expedition, some cases of remittent fever would occur even among the more cautious of the men; sometimes a debauch would determine the attack before this period, or bring it on at a somewhat later time in those who otherwise in all probability would have escaped. M. Littré, of the Institute of France, has ingeniously shown how the fatal illness of Alexander the Great was brought on in this way nine days after exposure to malaria in a boat expedition near Babylon.

Since concluding this paper the Essay of Dr. Gregory, already noticed, came into my hands; he gives illustrations of the incubation period of malarial or endemic fever in Sicily, from Sir William Franklin, Principal Inspector of the Medical Department of the Army. On the 12th of July, 1810, a regiment marched from Milazzo to the neighbourhood of Messina; a detachment was subsequently sent to occupy a large mansion, the Casa del Corso, where in certain seasons no one sleeps without afterwards suffering intermittent or remittent fever. Ninety-one men were

stationed here. On the 31st of July, thirteen days after exposure to the malaria, the first case of remitting fever was sent to hospital. On the 1st August, five others were reported. The following day the detachment moved from the Casa del Corso, and encamped near the rest of the regiment, which was then healthy. Yet the men continued to drop, and the admissions into hospital from this detachment were, August 2nd to 6th, one, eight, five, six, and seven men on each day; on the 7th eleven were admitted, and twelve on the 9th, after which the number were, four, eight, two, three, on consecutive days; then comes one on the 15th, two on the 20th, and one on the 26th day. Seventy-seven men of the ninety-one exposed to the germs of the disease were attacked; after their removal the greatest number of cases fell within twelve days, and one exceptional seizure takes place twenty-five days after removal from the malarious locality. Would the fourteen who escaped have fallen with fever had the exposure been longer? Dr. Gregory thinks not. He says, "as a proof of the intensity of the poison, of the seventy-seven attacked twenty-three died, being in the exact ratio of thirty per cent., the usual rate of mortality in small-pox, and also approximates to that of epidemic cholera." The account is insufficient to reveal the causes of this great mortality, or the exact nature of the disease. Like a true malarial remittent, it did not spread to any not exposed to the local influence.

In conclusion, the great questions bearing on all that may intensify or that can mitigate the extension and severity of these diseases must pass with bare mention. How much hygienic measures, directed by a full knowledge of the natural history of each epidemic, lessen the fatality of some and defend against others is beginning to be understood and acted upon. There are scourges of the past now only matter of history; social progress has slowly and indirectly, but most effectively, obliterated many evils inseparable from imperfect knowledge of the conditions and deficiency in the means of healthy living. Plague is abolished from Christian Europe. By what means the mortality from small-pox may be annihilated is well established; even if the reactionary activity of ill-instructed intellects so far prevail as recently in Leipzig, the head-quarters of German learning, to check the employment of vaccination; the loss in one year of seven hundred children, and the death of one in every hundred of the inhabitants by small-pox, forcibly recalls the value of the disused protection, and limits for the future

any such disastrous success on the part of anti-vaccination leagues.* Where we have no direct means of preventing personal susceptibility to disease, we lessen it by all that tends to maintain intact the vital resistance of each individual, by all that conduces to general prosperity, and by such local cleanliness and public conservancy as will maintain the purity and secure the freest supply of the water we drink and the air we breathe. The least speck of the less potent poisons finds quick inlet, and stings sharply when the state of health is weak, or the air bad around. Free admission of air almost robs the poison of typhus of its power; in my own experience this fever was banished for a time from London by the first operation of the Common Lodgings House Act. An infection the most subtle and persistent, that of scarlet fever, has been limited to one patient in a room with other children living and sleeping in it, by preserving a separation of a few yards, and attention to the maxim that disinfection must begin at the body of the sick. As these poisons are diluted and destroyed by fresh air, so are they intensified by accumulating in air otherwise foul; the mortality of measles is one per cent. under good hygienic conditions for all ages beyond the first year; under ordinary circumstances this rate is doubled; the usual mortality for all ages is one in thirty or forty cases; during the siege of Paris the mortality from measles in the Hôpital de Bicêtre exceeded one in three.† All clinical experience is confirmatory of Dr. Sanderson's demonstration of the increased virulence infecting poisons may acquire in transmission from one individual to another. Essential differences are shown in the various morbid poisons by another order of facts; the mortality from typhus is far less in childhood than in age; hooping-cough, so dangerous to the infant, is merely troublesome to the adult. The regulations to keep out cholera would be powerless for the exclusion of small-pox. Thus, the best intentioned sanitary efforts often fail, wanting the precise information that can alone be afforded by the science of medicine.

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* La Variole au point de vue épidémiologique et prophylactique, par Léon Colin, Professeur à Val de Grâce, Médecin en chef de l'hôpital militaire des varioleux de Bicêtre pendant le siége de Paris, 1870-71. Baillière et fils, Paris, 1873, p. 22. The numbers here quoted, given by Professors Wunderlich and L. Thomas, are from November 1870 to February 1871.

† *Op. cit.*, p. 151.

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INTRODUCTORY ADDRESS, SESSION 1873-1874.

W. R. E. SMART, M.D., C.B., PRESIDENT,
Inspector-General of Fleets and Hospitals, Royal Navy.

GENTLEMEN,—I thank you for the honour of being elected President of this Society on the roll of which there stand names deservedly high among epidemiologists. I am therefore sensible of your requirements from those you elect to occupy this chair, and shall not fail in doing my utmost to promote the objects of the Society, which are the ascertaining the causes, history, and best means of encountering those diseases that from time to time ravage communities and nations by extending from one to another. The more destructive types of these diseases have recently prevailed as epidemics of cholera, yellow fever, and plague; denguè has widely represented a milder and non-destructive type.

The most diffused of them has been Asiatic cholera in Europe, and over the basin of the Mississippi and Ohio. This, the most important of the prevailing epidemics, it will be useful and interesting to trace.

The history of this diffusion is but a continuous chapter with that of the invasion from Mecca, through Egypt, into Europe in 1865, which after an almost general European rest, in 1868 displayed itself as "cholera nostras" in some districts of west Russia, re-took epidemic type at Kief in 1869, and spread from it into the heart of Russia in 1870. In March, 1871, it reappeared in St. Petersburg and passed on to the shores of the White Sea, and to those of the Gulf of Finland. In July it was in Königsberg, from which it diffused along the Baltic shores, and, as we shall presently see, was carried from this point to our own shores and to America. By a reflected course into Asia it reached the Caspian in 1871, and, in the summer of 1872, the Russian station at Samarcand. It is remarkable that by a similarly reflected course the original outbreak of the Kourban-Bairam epidemic in Mecca, in 1865, was conveyed into

Persia; and in 1871, while it was marching eastward from Russia into Siberia, it spread westward from the head of the Persian Gulf across north Arabia back to Mecca, and was borne by negro pilgrims across the Red Sea into Nubia, and, we know not how far, into Central Africa in 1871.

While these renewals of the former invasion were progressive, the disease burst out afresh in India, where it had not been seen in epidemic form through 1870-71, but in the course of 1872 it carried off about 165,000 victims in the Bengal and Bombay Presidencies. From the former it spread through the north-west provinces into Cashmere, and onwards to Balkh, which was losing from 1,000 to 1,200 victims a-day in the month of August. The Russian possession, Samarcand, lying N.W. of it, separated by moderately high lands, had suffered greatly in July; so that it may be doubted whether Samarcand or Balkh formed the true demarcation of the reflected from the fresh epidemic, especially as the latter had waxed faint in Cashmere on the opposite side of the Hindoo-Kosh range.

We are accustomed to trace the westward course of cholera, and not until now has there been an occasion of noting its return into Asia from Europe; but there is no geographical law of diffusion, and even the eastward invasions of cholera from its cradle in Bengal are not without practical interest to Europe and America. On most occasions its westward extensions have been preceded, a year or so, by its spread eastward. Last year the disease was reported in Burmah, and this year it has spread in July to Bangkok in Siam and to Batavia in Java. In this course, by slow steps, it usually reaches Japan, and in our days, when commerce has opened up a great intercourse between Japan and San Francisco, as well as between the latter and China, through which large bodies of Chinese emigrants reach America, it would cause no surprise should cholera from Bengal *viâ* San Francisco meet with that from Europe *viâ* New York, as we have seen that from Russia meet with a fresh invasion from Bengal in the plains of Tartary.

After this recapitulation of the main features of this general re-diffusion of cholera from an European focus reflected into Asia until it came into near relation with a fresh trace of the disease from its Indian habitat, I shall enter on a closer survey of that wing of it which, taking a westward course, spread again over Europe and North America.

Reverting to the European epidemic, which I stated was

in Königsberg in July, 1871, we find it in the autumn so far west as Hamburg, in Hernosand, in Sweden, after visiting most of the ports on the South Baltic shore, and presenting itself in Berlin. From Stettin it was transported in an emigrant vessel across the Atlantic to Halifax, on the 6th of November, where it exhausted itself among the families of the pilots in quarantine. Into England cases were received twice in September without producing other cases. Late in the same year (1871) it was at the mouths of the Danube, and it wintered in Constantinople, from which it reached Salonica, in the Ægean, the island of Cerigo, Brusa in the Sea of Marmora, and St. Jean d'Acre; and in all these extensions it was attributed to introduction by passengers in steam packets from Constantinople. In all of these diffusions human intercourse may be assigned as the great factor, whether by regulated commerce at sea, or by the movements of troops or pilgrims.

In 1872, its activity was renewed in early spring at several points in south-west Russia, and by Midsummer it was epidemic in the wide plains north of the Black Sea, was present mildly in Moscow, and a little later in St. Petersburg. It broke out again violently at Kief on the 7th of June, raging amongst the pilgrims crowded in the courts and cells of the monasteries. In a southerly direction it ravaged the basins of the Dnieper and Dniester, and was in Odessa early in June, and in Constantinople in August. It traversed Bessarabia and reached the banks of the Danube. In a westerly line, the infection spread through Podolia into Poland and Galicia. It was at Warsaw in July, at Berlin and at Gefle in August, and in Sweden in October. North of the Great Carpathians it settled in Galicia, and extended into Silesia and to Saxony, reaching Dresden. South of the Carpathians, the epidemic crossing from Galicia, diffused widely in Hungary, and fixed itself in Buda and Pesth in the middle of October. It crossed the range also from Silesia into Moravia, and was in Prague, in Bohemia, 16th November. Vienna appears to have remained free in 1872. The infection wintered in Galicia and Silesia, in Hungary, Moravia, and Bohemia, having accomplished ten degrees of longitude westward from its winter quarters of 1871-72.

In January of the present year (1873) it quieted much in these provinces, but there were fresh outbreaks noted in west Russia, in Moscow, and provinces to the west of it, while Galicia continued to suffer inordinately from the

disease. It broke out too in Pultusk, lying north of Warsaw.

In May there was a grand revival in all the provinces infected by it in the autumn of 1872. It began to diffuse itself almost simultaneously along the banks of the Danube, Vistula, and Elbe, and it broke fresh ground in Venice and Treviso. On the Danube it began its march at the garrison town of Widdin in mid-May; upwards it appeared at Turnseverin, near Orsova, on the 24th of June, but it had been active up the stream at Pesth since the end of March. Down the river it appeared almost consecutively in Rustchuk, Giurgevo, Silistria, Kassos, Braila, Galatz, and Tultcha, on the banks of the river, and inland to some distance, at Krajova and Jassy on the north, and in Shumla in the south, infecting the garrisons badly; and it reached Varna, on the Black Sea. Constantinople escaped, but west of it the disease had spread to Salonica in September.

On the Vistula it revived at Warsaw in May, and from Thorn, where it was present in mid-June, was carried by the raftsmen to Dantzic at the end of June. Eastward of that city it spread along the Baltic to Elbing, Königsberg and Memel, with the opening of trade, and was felt at Cronstadt in August. To the westward it spread to Swinemunde, Stettin on the Oder, to Kiel, Copenhagen, and across the belt to Helsingborg in Sweden, and to Bergen on the west coast of Norway, in October.

On the Elbe it was present at Prague, from November 1872, with a short interruption in February, and it reappeared in Dresden in mid-May, and was at Hamburg and Altona in June.

In Vienna cases were reported in January, and probably cholera was never extinct in that city up to June, when it could no longer be concealed, but was assigned to fresh importation from Buda. It extended up the Danube to Passau, and from thence by the Iser to Munich in July. Passing from the Danube to the Maine it visited, in succession, Nuremburg, Bamberg and Wurzburg where it prevailed in September. In Berlin the first case appeared on the 21st of July, and the virus is not yet extinct.

After its outburst late in May in Venice and Treviso, it was at Trieste in July, and crossing Italy it appeared at Parma and Turin, and was in Genoa in mid-August, and it is stated to have been present since July in Naples.

To the west of the Rhine, there were rumours of its existence in Lauterburg, on the bank of the river between

Strasburg and Mannheim, early in July, but nothing has been heard since of its presence in that district or on the Upper Rhine. At Havre it was reported on the 19th of July, at Rouen on the 5th August, and at Paris on the 4th September, after infecting Caen, on the banks of the Orne. This visitation of France has been attributed to importation from Hamburg, and not from any extension of the disease from Germany by the land frontier.

In England cases have been brought up the Thames on two occasions—July 28th and September 6th—on the first among emigrants from Copenhagen, through Kiel and Hamburg, who on the 28th July were put on shore in east London where one died the next day. Without any delay all were sent on board a quarantine vessel at Gravesend, and no further results followed in London. Again, on the 6th of September, a steamer arrived from Cronstadt, which she had left on the 29th August, and a seaman had fallen sick on the 31st. The patient only was put into quarantine, and the vessel proceeded into dock.

At Southampton a case that proved fatal was admitted to the hospital and died same day, 10th September. He had been at Havre on the 6th September, the disease then existing there. On the 13th of September a case of sporadic cholera was reported at the coast-guard station at Netley, near Southampton, but it was not followed by others.

At Liverpool there were, early in September, four cases and three deaths among German emigrants, through Hull. In mid-September a fatal case occurred among French seamen from Havre, through Southampton, same day. Two vessels from Havre were docked on the 21st and 25th September; in the first vessel three of the crew, and in the last, one, died soon after arrival in the crowded docks.

It was semi-officially declared in the *Times* of the 15th November that, during the previous week, 6th to 13th, cholera had appeared here and there in many parts of Europe, fitfully and unexpectedly, and that up to that date, the ports infected with, or suspected of, cholera were Cronstadt, Memel, Königsberg, Dantzic, Stettin, Copenhagen, Helsingborg, Hamburg and Altona, Havre, Genoa, Trieste, and Varna.

By the earliest official report for November, 1873, for which I am indebted to Mr. J. N. Radcliffe, the disease was present in various parts of Hungary and Poland, and was not quite extinct in their capitals, Buda-Pesth or Ofen,

and Warsaw; the metropolises, St. Petersburg, Vienna, Berlin and Paris, were still infected.

On the shores and rivers of Europe there were then in quarantine, Cronstadt and Revel in the Gulf of Finland; Riga, on the gulf of that name; Lubeck, in East Holstein; Helsingborg, in Sweden, on the Great Belt; Bergen, on the west coast of Norway; Hamburg and Altona, on the Elbe; Rotterdam, Dort, and Scheidam on the Meuse; Antwerp, on the Scheldt; and Havre and Caen, on the Seine; in the Mediterranean, at Naples; in the Adriatic, at Fiume; in the Levant, around Salonica; and in the Black Sea, at Sulina, on a mouth of the Danube.

The outburst of cholera in the central United States simultaneously with its wide extension in central and western Europe in the past summer, has, from that circumstance, a cosmical rather than local interest. Its appearance was so unexpected that many have contended for its spontaneous origin there; and, if it were not imported, it will afford an instance of having been acclimated in the great central plains of North America, as some believe it to be in those of south-west Russia. But, on the other hand, it has been asserted that it was imported from Odessa into New Orleans, where it broke out in April, 1873, there being, however, no proof of its existence in Odessa immediately previous to that time; but it is of importance to recall that in the last quarter of 1872, the whole island of Cuba was in quarantine, with Jamaica, on account of suspicion, at least, of cholera, from which it is to be inferred that the disease may have been then in that region.

It appeared at New Orleans, and afterwards at Memphis, on the Missisippi, from which last it is said to have been carried on the railroad into Tennessee, infecting Nashville, in centre of Tennessee, by the 1st of June, and a belt of towns at some distance around it, as Gallatin, Murfreesboro', and Woodburn.

From Memphis it was conveyed in steamers to Evansville and Cincinnati on the Ohio, and to St. Louis at the junction of the Mississippi and Missouri. It had reached the uppermost of these cities—Cincinnati—so early as the 22nd of May, at the very time when its activity was being aroused on the several rivers before named in Central Europe. Its great characteristic was that, in all the towns visited, it broke out in the worst drained and least sanitary districts, and was most severely felt in Cincinnati, which is described by Dr. Peters, of the New York Board of Health, a resident

physician, as "a standing menace and danger to the rest of the country, more dangerous, perhaps, to other cities and towns than it is to itself and its own residents and visitors." Being the centre of the great bacon factories of the United States it is filled with piggeries, slaughter-houses, and salting houses. Its type therein was "most malignant and virulent, marked by suddenness of attack and rapidity of fatal result"—"creeping from person to person, and from house to house, with unmistakable marks of communication to relations, friends, etc., of the sick. As the drinking water is good, no general outbreak has occurred, and it has always been perfectly safe to visit". In this description there is a graphic instance of the spread of the disease in a city possessing the very unusual mixed sanitary features of filth and pure water. On the other hand, Louisville, lying between infected Evansville and Cincinnati, escaped the disease again for the fifth time, having had little or none of it in 1832-33, 1848-49, 1854 and 1866. It is "a very clean city, built on sandy gravelly soil, with broad, well-paved streets, widely separated houses, clean courts, alleys, yards, and stables."

This last chapter in the history of cholera, dating from its outburst in Kiev in 1869 up to the present time, epitomises the entire history of the disease, appearing also to substantiate the conclusion that, while it is capable of exclusion by preventive measures, it is also amenable, when introduced, to the laws that regulate the general health conditions of a region, district, or city; so that the general causes that enhance or impair those conditions become likewise the "*juvantia*" or "*lædentia*" in seasons of epidemic cholera; and that, where the isolated cases termed euphemistically "cholera nostras", are familiar in non-epidemic seasons, there true epidemic Asiatic cholera commits its greatest ravages and may probably even acquire epidemic force without any traceable fresh importation.

Further, that the almost simultaneous manifestations of the invasive form of cholera in May, 1873, at geographical points so far apart as Widdin, on the Danube, Warsaw, on the Vistula, Dresden, on the Elbe, the Venetian province, and New Orleans on the Mississippi, may be attributable to telluric or meteorological conditions proper to the season of the year, or to some pandemic constitution of which nothing is definitely known.

Lastly, amidst good local sanitary states, the infectiousness of cholera is at its minimum, as at Louisville, and

amidst the opposite conditions it is at its maximum, when it is scarcely distinguishable from direct contagion, as at Cincinnati, and in crowded garrisons as on the Danube, or in the ill-provided hordes that attend eastern fairs, and pilgrimages at Mecca, and frequently in India.

Opinions are still divided as to the best mode of dealing with cholera epidemics, and the practices in vogue aim at exclusion or palliation with the hope of eradication. The first of these was asserted as the only reliable method by the Sanitary Congress of 1866 at Constantinople, and it has been acted on with most beneficial results in Egypt and the Levant, at Malta and Gibraltar. The second is that which actuates us in England where trade scarcely admits of any beyond trivial restrictions.

Hitherto, the results of measures to improve our general health conditions, and prompt dealing with the first cases or units of infection, have been very satisfactory, so as to warrant a reliance on their efficacy; but there is one thing essential to complete success, viz., the utmost publicity with no concealment of local defects or of suspicious cases, which happily our habits ensure, whilst concealment prevails elsewhere. Thus, side by side, the success of these different principles of action corroborates the idea that in cholera epidemics, a "nidus" is as indispensable as a germ, but the broad questions are as yet *sub judice*. Is either of these essentials capable of producing the other? Can the germ originate *de novo* amid generally defective sanitary conditions? Or will the importation of a specific case into a generally healthy community cause the growth in it of the conditions needful to the development of the true epidemic type?

At the present moment another question presses itself on the public mind. What are the chances of an invasion of the present epidemic among ourselves? It is one that cannot be dismissed summarily, when we recollect that in the last quarter of 1831 the disease from Hamburg appeared in Sunderland in October, and in Tynemouth in December, to become general in May, 1832; that in 1848, again from Hamburg, it appeared in several parts of the north, but did not become general until April, 1849; that in 1853, once more it began in the north in September, and became epidemic early in the summer of the following year; and lastly, that in 1865 it appeared in Southampton, from the Mediterranean, in September, became epidemic in London in May, 1866, and in Liverpool in July. We have now in

1873 had the disease introduced into London from Hamburg and Cronstadt in August, and into Southampton from Havre in September; and it was conveyed to Liverpool from Havre through Southampton as well as direct into the docks from Havre by three several vessels, and into the town by emigrants from Hamburg through Hull—all in September.

Analogies like these leave grounds for the gravest apprehensions of the coming year, and press on the national and local authorities the necessity of urging to the utmost those hygienic principles on which we have placed reliance for the eradication or neutralization of the germs of the disease, which principles will have triumphed in the event of reappearance and extension not occurring among us in the spring of 1874. Our best assurance rests on the high state of public health marking the year 1873. The Registrar-General's report for the first quarter of the year states that:—"One of the most remarkable and satisfactory features of the period is the decline in the fatality of the principle zymotic diseases, of which, excluding small pox", that had swollen the previous returns, and was then receding, "the annual rate in the first quarter of the four years, 1870-3 has been successively 3·5, 3·0, 3·0, and 2·2 per 1,000."

In foreign climes we learn of small-pox in India and over Europe generally, and in the Azores, and Dr. Milroy acquaints me with the fact there has been a very great mortality by it through the summer and autumn in the island of Jamaica.

Small-pox commenced in epidemic form in England in in the last half of 1870, and showed no marked decline till the spring quarter of 1873, when the deaths had sunk to 722 since the corresponding quarter of 1872, when they numbered 7,720; and in the autumnal quarter the Registrar-General reports only 277 deaths, "which is probably the smallest number that has occurred since the beginning of the present century,"—"and only 15 among the population of 3¼ millions of persons living within the metropolitan area."

At the same time, measles and whooping-cough were declining, and among the principal zymotic diseases, scarlet fever and diphtheria were more than ordinarily fatal, and there "were many instances of excessive death rate and fatality from zymotic diseases in small villages and rural sub-districts."

The second quarter, to June 30th, was reported of as maintaining the steady decline in the fatality of zymotic diseases, and showing a death rate of 2·1 per thousand, against 3·2, 3·8, and 3·4 in the spring quarters of 1870-1-2. Small-pox, scarlet fever, and measles, had all abated.

It is most worthy of remark that the deaths from diarrhœa excited no notice by their frequency, or by any peculiarity, before the end of the first half of the year, when infantile diarrhœa began to prevail, and early in July a choleraic disposition was superadded in London. Thus, in the weeks counting from the 22nd of June to the 9th of August, the deaths rose steadily 22, 68, 100, 205, 305, 375, and 470, and from the 9th of August they as steadily declined on the following scale, 405, 362, 277, 229, 162, 127, and 85 for the week ending on the 26th of September, being then slightly below the corrected average in the corresponding weeks of the last ten years. The annual death rate from diarrhœa was equal to 1·3 per 1,000 in London; while in the 17 other large English towns it averaged 3.5 per 1,000. To cholera only three deaths were referred in the last-mentioned week, all being of children, certified as choleraic diarrhœa.

Since the spring of 1873, one of the most dreaded of endemo-epidemic diseases, yellow fever, has raged in the centre of North America and on the coasts of South America from Cayenne to Monte Video, without any apparent relation between those two epidemics. It was present in Monte Video in August, and it broke out in Shreveport, on the Red River, a western tributary of the Mississippi, at 400 miles inland from New Orleans, in August, and, later, at Memphis, which had already undergone an epidemic of cholera earlier in the season. Cholera spread up the river from New Orleans in April. Yellow fever had not been seen there since 1865, when it seemed to be eradicated from its perennial habitat by the vigorous sanitary measures instituted by General Butler. The fact of the existence of those two great epidemic diseases in the valley of the Mississippi in the same year raises suspicion of some widely-spread regional morbific agencies in operation there. The outbreak commenced in Shreveport, in Louisiana, where it was attributed to obstructions in the river at the mouths of the sewers, and afterwards it appeared at Memphis, on the main stream of the Mississippi 600 miles from its mouth, and then at Little Rock on the Arkansas River, through which the railroad from Memphis to Galveston Bay runs

to cross the Red River above Shreveport.* Later it has taken a westward course through Alabama, where the chief town Montgomery was suffering by latest accounts, and thence onwards through Georgia to Savannah, on the Atlantic shore.

Commerce has been suspended and town populations have fled before this scourge, the amenability of which to efficient sanitary measures has been confirmed in the island of Jamaica, under the guidance of a board of health, with its appointed officers located in the greater centres of a negro population.

The history of diseases amid the small communities of ships frequently yields instructive lessons, and one of such is given by the flying squadron. Of these ships, the Doris may be said to have had the least favourable conditions of general health. Being the worst infected by enteric fever she was detached at Barbadoes to convey those cases from the squadron to Bermuda, and having completed that duty she rejoined the squadron at Dominica in the middle of April. After visiting the Leeward Islands the squadron left Jamaica on the 28th May, between which and arrival off Nassau on the 8th of June, a fatal case of yellow fever terminated on board the Doris, no other ship presenting the malady, showing how much an inferior state of general health influences the susceptibility of a crew to yellow fever.

In Europe it has come to be generally admitted, after much controversy, that sewer filth and its gases are the generators of enteric fevers; and so, in America, the opinion is gaining strength, and will, I think, finally prevail, that filth, in the form of decomposing vegetable and animal matters, and the gaseous emanations from them provoked by heat and moisture, are the efficient causes of yellow fever. Whether the germ is or is not renewed, from time to time, in its usual habitats may be open to dispute, but all modern evidence goes to prove that it cannot take on epidemic intensity without the co-operation of heat and moisture on filth, and that places thus infected are dangerous for the healthy to enter,—such as the holds of ships deserted by their infected crews, and of buildings similarly circum-

* Subsequent advices from the U.S., at Memphis, where the ravages of yellow fever had been greatest, the disease causing 1500 deaths, declined at once, on the setting in of hoar frosts, and disappeared with the permanence of cold weather. So in all other towns the early winter put an end to the development of fresh cases and the disease speedily became extinct.

stanced on shore; and further to prove that although infection may be repeated by the removal of the sick into other localities where filth and heat and atmospheric moisture are combined, yet there is scarcely any danger where the sick thus removed, as from ships for instance, are placed in well-ventilated buildings, or in open tents, and their excreta removed, and that the vitality of the disease is destroyed by cooling of the atmosphere to the freezing point.

In the London district there has been a noteworthy outbreak of enteric fever this autumn in the best built and well-drained district of London, lying between Cavendish Square and the New North Road. In this instance, attention was directed by Dr. Murchison to the milk supply of the neighbourhood, which was traced to have been from the same dairy, one which professed to supply milk of faultless quality to its customers, and following out the inquiry, it was proved that on one of the farms from which its supplies were obtained the pump well was contaminated by leakage from a cess-pit, and that if no dilution of the milk could be asserted, yet the milk-cans had undergone ablution at the pump. The history of this case corroborates the views first started by Dr. Ballard to account for the outbreak of this fever in Islington in 1869, who with equal success applied his theory to account for the outbreak of this disease at Moseley, near Birmingham, in 1872. Consequent on this discovery, by examining thoroughly into the milk supply in instances of endemics of enteric fever, it has been shewn to be quite explanatory of similar outbursts, in Leeds, in 1872, by Dr. Robinson, and in Glasgow by Dr. Russell.

No further evidence is required, I think, to convince us of the accuracy of the theory that enteric fever is propagable by decomposing excreta conveyed into the stomach by the medium of drainage water. In all these instances the adulteration of milk with such impure water was most clearly to be inferred, as the connection between the cesspit with the water tank was proved, as well as the practice of using the water for washing the dairy utensils if not that of adding it to the milk for its trade dilution. Doubtless, there are ample grounds for legislation to secure a greater purity of this necessary of life, which is more than ever relied on as civilisation advances not merely as an ordinary article of diet, but as the chief *pabulum* for children, and for those who are reduced by wasting diseases.

An idea has been started that the milk produced by cows fed on sewage-farms is capable of extending enteric fever epidemics, but proofs are wanting, and it seems more likely that the frequency of entozoa may be increased by that agency, but of this result proofs are likewise wanting.

Whilst zymotic bowel disorders have been on the decline, and below par in England, where cholera has not extended from imported cases, there has been a proclivity to the fever types of disease, without excess in town populations over the general average of the kingdom. It may be inferred from this, that, in the towns, expenditure on sanitary works has been repaid by improved public health; and in reference to this the Registrar-General asserts: "This decline in the fatality of fever may be referred, in London, to an improved sanitary organisation, to a partially purified river, and to the greater purity of the water supplied by the metropolitan water companies."

In the etiology of cholera and of enteric fever, there is a presumed analogy that both are propagated by means of bowel excreta, with drinking-water for their vehicle. If this be true, yet the development of the diseases differs: enteric fever is found endemic in high or low localities alike, while cholera incubates on low marshy lands, and in littoral towns, and in streets along river-banks, from which it rises up to the higher grounds of towns, with mixed endemo-epidemic characteristics.

Interesting facts relative to enteric fever have been afforded in 1873, by two squadrons of our navy—the channel and flying squadrons. The channel squadron sailed from Plymouth on the 21st of October, 1872, and anchored at Vigo from the 25th of October to the 7th of November, leaving it without taint. The flying squadron left England in the third week of January 1873, and remained at Vigo from the 27th of that month to the 6th of February, leaving that port apparently healthy; enteric fever presented in two of the ships—the Narcissus and Doris—within fourteen days after departure.

The channel squadron returned to Vigo in good health on the 1st of March, and enteric fever presented itself on the 16th of March; the squadron remaining there until the 11th of April, with enteric fever in two of the ships. At each visit leave on shore was granted to the officers and men without hesitation, and all the ships replenished their water by means of tank vessels belonging to the town; and

the causes of disease must have been developed there between the date of departure of the channel squadron on the 7th of November, 1872, and the arrival of the flying squadron on the 27th of January, 1873.

The infection of the ships was attributed unanimously by their surgeons to the supply of water obtained there—in part from the town conduits, and in part from streams within two miles of the town, one of these coursing through a district of vineyards undergoing manuring at that season, and both being resorts of the washerwomen of the town.

Vigo is surrounded by mountains, and is placed on a hill-side, with sewers running along the middle of the streets, broken into in many places, giving exit to foul odours, but undergoing a natural flushing in the winter season during the heavy falls of rain. The surgeons failed to assign more than one case of fever to communication with the town, and indeed some were attacked who had not landed, while all drank of the water received on board.

The health of the town was reported to be *particularly good* at the time; but typhoid cases were said to prevail there occasionally, and in the previous year our consul had lost a member of his family by it.

Out of seven ships, four were attacked; and of the three exempted, two (Aurora and Endymion) were furnished with means of filtering the drinking water; no reason so probable has been offered to account for the exemption of the third, the Topaze, but it is possible that her water may have been derived unmixed from the purest of the three sources from which the squadrons were supplied. The water appeared after sediment to be free from any perceptible impurity, and was free from any disagreeable taste.

After the flying squadron had proceeded to sea, the fever appeared on the fourteenth day, continuing to afford cases up to the thirty-fifth day in the worst infected of the ships, the Doris. On their arrival at Barbadoes, the authorities declined to receive the sick on shore, which led to their transfer to the Doris, which proceeded straight to Bermuda, where thirty-two cases were landed next day (17th of March) at the quarantine island appertaining to the Naval Hospital, where one death occurred, and the remainder convalesced.

The channel squadron took a homeward course from Vigo, arriving at Portsmouth on the 15th of April, and

landed at once thirty-five fever patients, and subsequently seven cases, making in all forty-two cases of fever. Of these, three died and thirty-nine convalesced.

The Minotaur sent her last case to hospital on the 1st of May, the sixteenth day after arrival; and the Sultan hers on the 9th of May, twenty-four days after it, and this last case was probably due to infection at Portsmouth, where the disease existed.

From this history may be deduced:—

1st. The necessity for a chemical examination of water received into ships, especially in foreign ports, as well as inspection of the sources.

2ndly. The advantage of using some efficient filtering apparatus in every ship, for purification of the water for drinking purposes.

3rdly. The benefit to a sickly squadron in having a special naval medical depot to resort to, from which local authorities have not the power to debar them admission, as occurred at Barbadoes.

Epidemiology has, I believe, a wide field open to it in the differentiation of the many forms of causation and development of fevers in tropical climates, which are very unlike in the large cities and in the uncultivated tracks of country. In the former I have seen much of what I consider the enteric.

Naval medical history has afforded this year another instance of what, in the public mind, has been regarded as one of epidemic disease; whereas, in reality, its features are purely endemic: I allude to the mortality among the Royal Marines, hastily despatched as a trusty body of soldiers, ever ready to embark without that preparation for foreign war-service required by regiments of the Line.

On the 13th of May a detachment of six officers and 104 men left Portsmouth and reached Cape Coast Castle on the 7th of June, and disembarked there on the 9th, into the narrow quarters of a small fortress, in which the water supply, obtained from the roofs and stored up in underground reservoirs of the Castle, is stated to have been disagreeable to both smell and taste, but used by thirsty heedless men, unacquainted with the dangers they ran, while for rations, they received, through a week or ten days, salted meats not of good quality, rice, and defective bread, with means of cooking inadequate to their numbers.

The exigencies of the Service demanded from a con-

siderable proportion of them a march of eight miles to El Mina, with increased hardships. Climatic sickness began to show itself among them on the second day, but not generally before the third week, until at length their condition was such as to compel a re-embarkation for return to England in the eighth week from their arrival, having already lost two of their number.

Of the original strength, 110, two having died, and twenty-one (four officers and seventeen men) remaining at the Castle, the others, eighty-seven in number, were put on board on the 1st of August, and returned to Portsmouth on the 26th, having lost ten men at sea. Thus reduced to seventy-seven, of whom eighteen having recovered, proceeded to their barracks, and fifty-nine being still sick, were sent to the naval hospital, forty suffering from remittent or intermittent types of fever, and nineteen from dysentery, of whom one died of the former and three of the latter disease.

The diseases to which these brave men fell victims are not peculiar to the African shore, but the same as Army and Navy medical officers are familiar with in all damp tropical climates, and as purely endemic there as agues and enteric fevers are in certain parts of England.

The fever type is the malarious-remittent, in the degree rightly styled "pernicious," ending sometimes fatally by coma, and changing to the intermittent degree in convalescence.

The dysentery is of the inflammatory form, with enlargement and ulceration of the solitary glands of the large intestines and extensive heterologous exudation into the submucous tissue, leading to ulceration and sloughing of the superjacent mucous coat, and sometimes complicated with abscess of the liver, as was displayed in one of the three cases of the disease fatal in Haslar Hospital.

Although the majority of the cases had exclusively one or other of these forms, yet the diseases were frequently intercurrent; commencing as remittent fever they took on dysenteric symptoms, or as dysentery they lapsed into remittent fever, indicative of one causation for both forms of disease. It was noted in one of the fatal dysenteric cases, that the blood in the left ventricle was filled with bacteria, and that in another, in which the liver was reduced to a unilocular abscess, the contents were replete with bacteria and bacteroids; and in all the fatal cases, whether of remittent or of dysentery, that the spleen presented the uniform characters

of great enlargement, and a soft friable parenchyma of deepest claret colour, giving the best pathological evidence of a paludal causation in both forms of disease.

It is remarkable that while small-pox raged among the negro population crowded around the castle, no case occurred among the marines within it, proving the efficiency of the system of vaccination enforced at their head-quarters before embarkation. Protected from a raging zymotic epidemic disease, they succumbed to equally preventable endemic diseases. It may be hoped that, being the forerunners of a greater expedition, their sufferings and losses will bring comparative security to those who have followed them.

Another febrile epidemic disorder, as yet unknown in Europe—the denguè, or broken-bone fever, from the severity of the pains in the back and limbs attending it, has continued its eastward march during the year.

This eruptive disease is, like influenza, a pure epidemic, first attacking a few, and extending its circle until nine-tenths of a community into which it is introduced feel its force. It fills all with great alarm by the violence of its assault, prostrating its victims with febrile pains followed by a more or less intense scarlet eruption, it ends without any mortality or any sequelæ, and does not secure its sufferers from second attacks. From the middle of the last century its outbursts have frequently occurred on the opposite side of the Atlantic, and I have witnessed two such epidemics in Bermuda between 1860 and 1864.

It was severely felt in Calcutta in 1824, and was described by Dr. James Mellis in the "Transactions of the Medical and Surgical Society of Calcutta." From that time until 1870, there is no record of its appearance to the east of the Cape of Good Hope: but towards the end of that year it appeared in Zanzibar as an unknown disease. It is remarkable and significant that the name there given to it by the African natives was ka-dingà, which is strangely suggestive, either of derivation from the West Indian term denguè, or that the African name ka-dingà had originally found its way from Africa to America among negroes imported as slaves from Africa.

After its appearance at Zanzibar in 1870, it spread to Aden in June, 1871, believed to have been introduced from Zanzibar by native trading craft. It then extended to Bombay, and was present a second time in Calcutta in September 1871, and became widely diffused in Bengal in

1872, causing much alarm and distress. In 1872 it was in Madras and in the south of China, and by late accounts it was raging in Shanghai and adjacent province in June, 1873. In 1872 also, the pilgrims at Mecca were said to be suffering an epidemic of it.

A good example of its diffusive power is related in the case of a vessel—the Dalhousie, which, between Aden and Bombay, had the disease among her crew. At Bombay she was cleansed and painted, but not disinfected. She there embarked troops for Cannanore, on the Malabar coast, where she arrived on the 2nd March, 1872, the men were then healthy, but the disease broke out among them immediately afterwards, when only four escaped, and the disease then spread across the presidency to Madras.

This, gentlemen, completes my cursory survey of the broad paths followed by the epidemics of the past year. Before closing, it is meet that we should pay the passing tribute of a sigh to those of late fellow-workers whom death has removed to scenes beyond his own domain.

I cannot doubt that every member of this Society heard with deep regret of the untimely removal of one whose cheerful, intelligent countenance was now and then seen, and his voice heard in discussion, at our meetings—Dr. John Murray, than whom none could display a higher regard to the dignity and true interests of our profession, nor a keener spirit in pursuit of that practical knowledge which ennobles it. He has fallen an early victim to the fell diseases which he strove to make his special study, as has frequently happened to many who in their youth had gained the applause and won the confidence of their professional brethren. And of him it may be truly said, "non tetigit quod non ornavit"; and while we sympathise in the irreparable loss to his family, of whom there is amongst us one we must recognise as a veteran in medicine, entitled to our highest regards as an epidemiologist, who also viewed with fond pride the reputation of his kinsman bearing his name, walking in the path of professional rectitude with promise of a bright future, so we may hope to be allowed to sympathise specially with him in his bereavement, with feelings deeper than words can express.

We have also, as a society, to lament the loss of Mr. W. D. Michell, whose zeal in our special pursuits was well marked, possessing an intimate knowledge of the march of epidemic diseases in the vast empire of Russia. He was

elected to fill the office of our secretary for that region, and he is taken away before we have reaped the fruit of his labours.

However much we may lament, it is not for us to recall them; but let us hope that they will not be entirely forgotten so long as honesty of purpose shall mark the endeavours of, and be esteemed by, our society.

ERRORS IN THE USUAL METHOD OF INVESTIGATING THE CAUSES OF EPIDEMICS.

By ROBERT LAWSON,
Inspector-General of Hospitals.

[*Read March 11th*, 1874].

There is a great divergence of opinion among epidemiologists as to the causes of certain diseases which spread widely from time to time, though the conclusions they arrive at be drawn, ostensibly, from the same facts, and far less progress is made in narrowing the points in dispute between them than might fairly be expected when such activity exists as among the supporters of the different views. It appears to me this divergence is attributable, in no small degree, to the employment of faulty methods of investigation, whether in the weight attached to certain data, or in the reasoning which is based upon them. The subject merits the attentive consideration of the society, and, on this occasion, I will mention a few of the errors encountered at the very threshold of an investigation into the causes of epidemics, with the object of eliciting their views.

When a certain result, E, is found to follow the operation of any influence, which may be designated C, and when E is never met with unless preceded by the operation of C, then these two phenomena are said to stand to each other as cause and effect. When C is an isolated phenomenon, there may be no great difficulty in establishing its relation to E; but if, as is very common in the processes of nature, C be associated with other phenomena, which may be indicated by A, B, and D, the question becomes more difficult, and trustworthy evidence of the influence of C can only be obtained by eliminating that of the others, by repeating the observation under circumstances varied so as to exclude one or more of them at a time, until C alone has been the only one constantly present. Again, though C may be the

immediate cause of E, without the intervention of which E never presents itself, yet some of the other concomitant phenomena, A, B, or D, may be necessary to ensure the operation of C; or, again, they may give to the influence of C some peculiar extension, or other characteristic, the cause of which it becomes necessary to investigate. This can only be effected by a process similar to that already indicated for the more simple case; but, of course, extending the analysis to all the possible factors, and to their relative influence in ensuring the result. Such is the method by which the marvellous advances in the exact sciences have been made, and it is the only one by which epidemiologists can extricate themselves from the chaos of opinion which now hampers them. In the exact sciences, it is true, many constants and fewer variables are met with, which serves to simplify investigations, while in epidemiology, so far as our present information goes, there are fewer constants and many variables, which of course complicates them: but instead of this being a reason for relaxing the rigour of the method, it is really the strongest that can be advanced for strict adherence to it, and no sound progress can be made until epidemiologists generally acknowledge the necessity for pursuing this course, and decline to receive as established scientific truths inferences which are not supported by it. They will frequently have to do with opinions for which this basis of certainty is wanting, but in such cases the views should be set forth as probabilities, or possibilities, only, and not as well-established truths.

There is a distinction between physical and epidemiological investigations which requires to be noticed here. In the former many of the factors can be isolated, and their properties examined, and if, in the case of others, they cannot be obtained in an isolated and material form, their properties can still be demonstrated by experiment quite independently of the act of causation it may be desired to determine. In epidemiology it is quite different, the peculiar poison or excitant of typhus, enteric, or relapsing fever, of intermittent, remittent, or yellow fever, of plague, cholera, scarlatina, measles, or hooping cough, has never yet been obtained in a separate form, and the existence of either, even, is only inferred from the occurrence of cases of the corresponding disease; in short, there is no reagent for any of these, save the system, whether of man or of such of the lower animals as are susceptible of the particular form of disease; and though our experience may lead us to suspect

that, under certain circumstances, one or other may be present, still there is no means of determining whether this be so, unless the employment of the test, followed by the characteristic reaction, in other words, of the manifestation of the disease among persons or animals exposed to its suspected cause. It is obvious this peculiarity greatly enhances the difficulty of solution of epidemiological questions, by requiring that the causes should be traced back from the effects, while it is impossible to obtain corroborative proof of their operation, by independent means, at any point of the process.

An immediate consequence of this peculiarity has been the variety of causes assigned by different investigators for the same disease—cholera, for instance. It is not surprising that men of ardent mind, anxious to avert the ravages of this pestilence, should have been led away by some new fact, and, under the disadvantages of a limited field of experience, and want of information as to its natural history, been induced to form generalisations which more extended observation, and a more developed natural history, show to have been partial, or even erroneous, from the authors having omitted all consideration of one or more of the factors engaged in bringing about the results they met with, or from their having overlooked the true causes altogether. It is clearly essential that this error should be avoided, and whenever a supposed cause does not account for the result, the operation of some other factor or factors must be looked for, or the hypothesis modified so as to cover all the details satisfactorily.

It is necessary to allude to a distinction which has been made between what is called positive or affirmative evidence, and negative evidence, some attaching much greater weight to the former than to the latter in deciding questions in etiology. One observer, having seen a given disease arise under certain circumstances, forms a theory as to its causes in accordance therewith. A second, having met with the disease under other circumstances, adopts a different theory. Either calls that combination of circumstances, which is corroborative of his theory, positive or affirmative evidence, when followed by the disease, and attaches a high value to it; but when the other points out to him instances in which the combination he deems sufficient to ensure the usual result was in operation, yet the result did not follow, the first forthwith designates it negative evidence, and of little value as compared with the positive. Either party

may adopt this line of argument with equal justice, but, after all, he who does only takes such facts as suit his hypothesis, and ignores those opposed to it. There may have been some important factor undetected by both parties, or they may have included some that were not concerned; and how is that to be found out unless by carefully considering the negative instances, and endeavouring to ascertain wherein they differed in their antecedents from the positive. It is clear that a theory which cannot account for the negative, as well as the positive results, must be imperfect, if not actually erroneous, and the true spirit of induction requires us to regard the positive and negative results as equally worthy of attention, and as equally important in guiding us to a correct conclusion.

From the days of Sydenham there has been a prevalent belief among the members of the medical profession, that, from time to time, an epidemic cause or constitution comes into operation, under the influence of which not only the attacks of particular diseases are greatly increased in number, but generally also their nature aggravated. The terms "epidemic cause," "epidemic constitution," seem generally to have been employed to embrace all the influences which contributed to produce the outbreak, save those immediately connected with the population amongst which it occurred, or the locality they inhabited, or matters connected with the disease (such as the virus of small-pox, when it was a question of that disease), clearly introduced into that locality from elsewhere. These may be indicated in language more in accordance with our present ideas, as one or more factors of occasional, though often very extensive, operation, but of the nature of which there is little or no information. It is not astonishing, therefore, that in all the controversies regarding the way in which epidemics arise amongst certain populations, so little reference should be made to these factors, though it is obvious that, where they are concerned, speculations regarding causation which do not recognise them, and allow for their effects, must be unsound, inasmuch as they do not bring into view all the causes in operation, and hence must attribute to some more influence in producing the result than they have really exercised, or may even have assumed the co-operation of others which were not engaged at all.

The influence of the epidemic cause may be illustrated by small-pox. This disease, as all are aware, can be communicated readily by inoculation, and hence many have concluded

that all that was necessary for the generation of an epidemic of it was merely the introduction of the virus amongst a population insufficiently protected by vaccination, or by having undergone the disease. Is this opinion borne out by what is known of the cause of small-pox? In the annual summary of the Registrar General, of births, deaths, etc., in London, in 1870, there is given, at page 16, a table of the weekly deaths from small-pox, from 1840 to 1870 inclusive, a period of thirty-one years. During this there were but fifteen weeks without one or more deaths from small-pox in London, and, up to 1869, there were eight distinct epidemic outbreaks, of various intensity, separated from each other by intervals of from twelve to twenty-four months, during which there was comparatively little of the disease. It is here to be remembered—1st, that for these thirty-one years the virus was never absent from London; 2nd, that notwithstanding its constant presence, epidemics recurred only about every fourth year; consequently there must have been some factor or factors of occasional operation concerned in developing them. The mere exhaustion of susceptible subjects, so far as that depended on absence of, or insufficient, vaccination, or of protection by a previous attack of the disease, cannot account for its decrease of force at the end of each epidemic period, for many of both descriptions of persons, who passed through earlier epidemics untouched, were attacked during that in 1871-72. Certain conditions of the weather, and insufficient nourishment, whether depending on depressed trade, or high price of provisions, may, and do, assist other causes in predisposing to any epidemic, but of themselves they cannot develope small-pox, and their recurrences since 1840 do not coincide with the epidemics mentioned above; while, during the last and greatest, that of 1871-72, trade was unusually prosperous, and the population in comfortable circumstances. To what, then, are these epidemic outbursts of the disease to be attributed? The information at my disposal regarding those before 1869 is not sufficiently full and connected to bring this out clearly, but for that which commenced in 1870 the evidence is more complete.

In the second quarter of 1869 small-pox reached its minimum in London, the deaths from it amounting to fifty-five only; from this they increased to 157 in the third quarter of 1870, in the fourth they rose to 584, in the first quarter of 1871 there were 2,400, and in the second, 3,241, from which they declined to the last quarter of 1872, when they

amounted to 131 only. The mortality from small-pox in England and Wales fluctuated as much as that in London; the deaths were at their minimum in 1869, when they were only seventy-two in one million living; in 1870 they rose to 118 in a million, and in 1871 to 1,024 in that number. In Scotland much the same was observed; the deaths from small-pox were lowest in 1868, when there were only fifteen recorded in the country; in 1869 they amounted to sixty-four, in 1870 they rose to 114. The returns for the country at large for 1871 are not yet available, but in the eight great towns there were 880 deaths from this disease that year, and in 1872, 1,537. Thus, not only in London, but over England and Scotland, the same disposition of small-pox to increase is manifested in 1870, and greatly developed in 1871.

Coincident with the epidemic in London, there was a severe one in Holland. I have not been able to find out the exact date of commencement, but in the four principal towns there was a considerable mortality from small-pox in December, 1870, which was considerably increased in January, 1871; the deaths attained the highest point at the Hague in January, at Utrecht in February, at Rotterdam in March, and at Amsterdam in June; after which dates, in each case, they diminished in numbers, and in July, with the exception of Amsterdam, had become comparatively few. In the course of 1871, Brussels, Berlin, Vienna, and other parts of Germany, and France, suffered severely from small-pox. This disease also became epidemic in Philadelphia and New York in the course of 1871, and in the end of the year, or early in 1872, it invaded Canada.

Effects so extensively diffused as those just described are only to be attributed to causes of equally extensive operation; the mere presence of the virus, as has been shewn in the case of London, altogether fails to explain them, neither can any other of those local or temporary circumstances, which affect limited districts, account for the aggravation of disease far beyond the sphere of their influence. Whether this epidemic or pandemic cause embrace one or more factors, it is impossible to say at present; neither is their mode of operation known, though certain facts seem to indicate that, like epidemics of fever and cholera, those of small-pox move from south to north, and that during the last epidemic, under these influences, the susceptibility of the system to the action of the vaccine virus was observed to be remarkably increased in persons who had undergone vaccination or small-pox on previous occasions.

It has been objected to the reception of the epidemic cause, first, that, though chemical and physical means of research are now very delicate and refined, they have entirely failed to show any condition of the atmosphere, or of the magnetic currents of the earth and air, which can be so unnatural as to produce disease; and, secondly, that, were such a general condition in existence, every place within its sphere of operation should manifest its influence to some extent. As to the first objection, it must be recollected that, as stated above, no chemical or physical experiment has as yet enabled us to shew the existence of the active causes of many diseases, though the recent progress of these sciences favours a belief that a time may come when they will elucidate this question, as they have done so many others, that but a few years ago were deemed inexplicable. Until then, however, we can only employ the reagent we know of, viz., the human constitution, and endeavour to elicit the causes by carefully considering the results. With regard to the second objection, it must be remembered that, if we wish to advance our knowledge of the laws of nature, we must avoid attempting to fix the limits, or mode, of her operations by *a priori* reasoning, but must try to arrive at these from the phenomena she presents for our observation. Now a reference to the returns of the Registrar-General will show that in the small pox epidemic of 1871 there were various points in the country where it acquired a high degree of intensity, while, at the intermediate places, it displayed every variety in its frequency, from numerons attacks to almost entire immunity, though the epidemic cause embraced the whole. This feature is observed in every epidemic, and in every country, and must therefore be accepted as the true mode of operation of the epidemic cause.

I have selected small-pox to illustrate this question, as the disease which presents the facts in the best form for the purpose. It being one which all agree can be communicated from man to man, and of the course of which in this country, for some years past, there are trustworthy records, it was easy to show that the mere presence of the virus was quite insufficient to account for the recent severe epidemic without the concurrence of the general cause. It must not be supposed, however, that small-pox is the only disease so influenced; the whole class of fevers, cholera, hooping cough, and several others, present similar characters, and our attempts to account for the appearance of these diseases, under many circumstances, will prove as un-

satisfactory as they hitherto have done, until we consent to recognize the operation of the epidemic or pandemic factors, as well as those more commonly treated of.

In our enquiries into the causation of certain diseases it becomes necessary to determine whether they were introduced from elsewhere, or arose from the action of causes at the place when they appeared. This, of course, is to be effected by eliminating the influence of the local causes, but few, perhaps, have considered the measures necessary to secure its exclusion. Let us take the case of yellew fever. As mentioned above we have no reagent for the yellow fever poison save the bodies of men, or animals subject to it, consequently, when we see fever arise in any locality, that fact merely indicates that the causes are active there at the time. If, previous to the occurrence of cases in the locality, others had been introduced from elsewhere, that would raise the possibility of their being due to something communicated from those introduced, but still it is impossible, in such an instance, to separate the local influences from the personal, so as to obtain decisive evidence on this point. If from this first locality cases of the disease be carried to another, and other cases appear there soon after, the same difficulty recurs—the result merely substantiates the fact that the causes of the disease were in operation in the second locality at the time, and, however frequently this combination may be repeated, it proves nothing more. The influence of locality cannot be excluded in yellow fever after the introduction of cases from elsewhere, unless no fresh ones show themselves among persons who have not been away from the place for some time; under such circumstances it is obvious there is no evidence of local causes being in action, as no disease is produced, and it then becomes possible to deal with the question of personal influence by itself. The following instance serves to illustrate these positions and to show their importance :—

In 1865, yellow fever occurred at Sierra Leone, and the Isis, receiving ship, which had been there for some years, had several cases on board, of which the last two were attacked on the 16th and 18th December, respectively, and both died on the 21st. A few days after this H.M.S. Bristol, a steam frigate of 3027 tons, with a crew of 535, arrived at Sierra Leone from England, and, instead of running up to Freetown as usual, anchored off the lighthouse in the open sea, some five miles from Freetown. It being considered that the position of the Isis was an unhealthy

one, on the 28th December a party of four officers and 112 men were sent from the Bristol to weigh her anchors, and remove her to another; as the work was not completed that day the party returned on board the Bristol at a late hour, and went back to the Isis on the 29th, and finally returned to the Bristol the same night. Two officers of the Bristol had landed at Freetown on 26th Dec., and having missed the boat which was to carry them back went on board the Isis, where they passed the night. One of these was attacked with yellow fever on the 29th, the other escaped with a slight feverish attack only. Some men went from the Bristol to Freetown on 29th Dec. to purchase mess traps, one of them was on board the Isis a quarter of an hour, and suffered from an attack of yellow fever subsequently. Of those who went on board the Isis 2 were attacked on 31st Dec., 20 on 1st Jan., 1866, 6 on 2nd Jan., 3 on 3rd, 3 on 4th, 2 on 5th, 1 on 6th, and 1 on 12th; making a total of 38 cases, of which 29 were classed as yellow fever, and nine as remittent. Of these there died, 1 on 3rd Jan., 4 on 4th, 6 on 5th, 4 on 6th, 3 on 7th, and 3 on 10th, making a total of 21 on board. Two died subsequently at Ascension. In the cases returned as yellow fever the urine was found to be highly albuminous in every instance in which it was examined, loaded with tube casts, epithelium, and blood cells, and black vomit was frequent, so there can be no doubt from these symptoms, as well as from the rapid course of the disease, and from the enormous mortality, that it was truly malignant yellow fever. It may be a question whether the nine cases classed as remittent were not of the same nature as the others, only milder in degree, as at first the symptoms were the same, though on the second day there was a slight remission, and generally a perfect one on the third, but there was a slight amount of albumen in the urine in some of them; whether it were looked for in all is not stated in the original account.

All these cases manifested themselves in the Bristol, and though many officers and men who had been on board the Isis escaped, still no one was attacked who had not been exposed in her to whatever cause for the disease was there. With reference to the exposure of the rest of the crew of the Bristol to the sick, the surgeon says in his report: "Had the fever been a contagious one it had ample opportunity of propagating itself, for though the precautions taken to separate the sick from the rest of the ship's company were the best that could be adopted under the circumstances,

still they were inadequate to have prevented the spread of a contagious disease. And again, the men employed as nurses, numbering some twenty-four, who were hanging for hours over and supporting in their arms their dying shipmates, their frocks in many cases accidentally covered with black vomit, and treading with naked feet in the black vomit about the deck, none of these men suffered from fever; nor did any of the medical officers, five in number, who were in constant attendance on the sick, suffer. Also, on the removal of the sick to the hospital at Ascension, not a single case occurred either among the nurses or the medical officers of that establishment."*

On analysing this case these facts come out distinctly—1st, That the Isis had become a focus of the disease, though whether from a contagium left by the preceding sick, or from something in the vessel herself unconnected with these, the evidence does not permit us to decide. 2nd, The observation was repeated in a new locality, on board the Bristol, where the non-occurrence of yellow fever among her crew, either up to this time or subsequently, enables us to state there was no evidence of local causes being in existence, and the result, as mentioned above, was the complete failure of the fever to propagate itself, notwithstanding the large number of serious cases under treatment at once, and the free exposure of the attendants. Of course if the fever could not propagate itself in the Bristol, or at Ascension, there is no evidence of its having possessed that power in the Isis, as, if a contagious disease in the latter vessel, it should have retained this character in the former, when the opportunities for its developing it were abundant.

This is by no means an isolated case in the history of yellow fever; there are many others, both connected with ships and on shore, in which the evidence is equally clear in favour of the influence of locality, and the non-operation of contagion. The account of the outbreak at Newcastle, in Jamaica, in 1865 (*Brit. and For. Mid.-Cher. Rev.*, Oct., 1859, p. 445), is an example. On the other hand, there are many instances of yellow fever having appeared among the population of localities where it shows itself at variable intervals after the arrival of cases from another place, in which the evidence does not admit of the influence of local causes being separated, yet it is almost universally on such occurrences that those who advocate contagion in this

* "Report on the Health of the Navy, 1866," p. 226.

disease base their argument. Had the men from the Bristol who contracted yellow fever in the Isis remained in the latter, or had the Bristol contained local causes (similar to those which produced the fever in the Isis), ready to come into operation, and others of her crew than those who had visited the Isis become affected, how completely useless the evidence would have proved for settling the question of contagion either way, simply from the impossibility of separating the operation of the local element. Though the illustration in this instance be taken from yellow fever, yet the principle involved, as in the more general one of epidemic or pandemic causes, has a more extended application, and requires to be observed more or less in all etiological investigations.

Occasionally there is a great divergence of opinion between different schools of epidemiologists as to the weight to be accorded to certain cases, which for the most part occur either singly or in small groups, and which present a close resemblance to those we meet with at other times in large numbers constituting an epidemic. This point may be illustrated by cholera. Two principal forms of this disease are recognised, viz., the summer, English, or simple cholera, or cholera nostras, and the Asiatic, epidemic, invading, or malignant cholera, as it has been variously called. The former is characterised by bilious vomiting, the stools seldom lose the fæculent appearance entirely, the collapse is seldom severe, the urine is not suppressed, and the mortality is small. In the latter all this is reversed, the matter vomited, or passed from the bowels, has the appearance of rice water, the collapse is most pronounced, the urine usually suppressed, and the mortality very great. The simple cholera, as its synonyms imply, is most frequent during warm weather, and is by no means confined to any one country. Cases appear in England, as elsewhere, more or less every year, but in some years in greater numbers than usual, and on these occasions there are generally seen a few others presenting all the characters of the malignant form, and which, had they occurred in the middle of an epidemic, would have been acknowledged as typical instances of the disease. One class of epidemiologists adduces these cases as proof that cholera of the malignant form may arise without previous importation, while another, which deems importation a necessary factor in the production of an epidemic, relegates them to the division of simple cholera, but without assigning any proof, that I am aware of, of suffi-

cient weight to justify their so doing. Here is a point at the very root of the contention, and no settlement can be made which does not include it, consequently it is opposed to the spirit of scientific enquiry to set it aside unless on the clearest evidence that the nature of these sporadic malignant cases really differs from that of those which we meet with when the disease is epidemic. Every disease is characterised by certain symptoms during life, and after death, appearances peculiar to it are frequently to be found in various parts of the body, and, conversely, when we meet with this combination of symptoms, and, still more, when these are supported by the usual post mortem appearances in the various organs, there is no alternative—unless we agree to abandon all attempts to distinguish particular forms of disease—but to refer these symptoms to their appropriate complaint, under whatever circumstances they may present themselves, and whether sporadic or epidemic. It may be that a certain similarity between the symptoms of two distinct affections may lead us to consider them as identical until precise investigations establish their difference, but until these investigations have been made, and their results well established, it is clearly contrary to the spirit of scientific research to set aside one series of facts which are opposed to a particular conclusion, while another series is adopted which may be favourable to it, though hitherto every attempt to draw a distinction between them has failed.

REMARKS ON THE CONDITIONS OF PROPAGATION OF SMALL-POX AND CHOLERA.

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[*Translation: read April 8th,* 1874].

I.—The great extension of the last epidemic of small-pox allows us to compare the conditions of its development with those of cholera. These two affections present, in fact, the common character of being the diseases which, in this age, are the most widely spread over the face of the earth. Nevertheless, until now, they have been separated the one from the other, not only because of their symptomatic differences, which we pass by, but also because of their *mode* of propagation: the one, small-pox, is usually considered as exclusively contagious, almost independent of the topographical and hygienic conditions of the place where it is developed; the other, cholera, on the contrary, is cited among the epidemic diseases which are most strictly subordinated to these local influences.

The epidemiological study of these two affections allows us, however, to recognise, for the one as for the other, the simultaneous intervention in their propagation of two different factors: first, the human organism; second, an influence from without overruling this organism—an influence constantly spoken of as epidemic.

II.—The part played by the human organism in the propagation and generalisation of the epidemics of small-pox is so evident that we do not entertain the thought that this disease can actually appear, without human importation, in any locality where it has not before been known. In our own country we have had proofs quite recently of the influence of human communications: first, by the extension given to the late epidemic in France from the movements of troops occasioned by the war in 1870; second, by the

penetration, out of France, and specially in England, Switzerland and Belgium, of the germs exported by the civil population removing from the theatre of war.

The part played by the human organism, in giving rise or extension to epidemics of small-pox, can be notably diminished by certain individual conditions, chief among which are: first, protection by vaccination; second, protection by a previous attack of small-pox. In my work on this small-pox epidemic I have given fresh proofs of these two protective conditions; thus, during the siege of Paris the capital received from various points of France both regiments and groups of the civil population; now, the small-pox prevailing at Paris, developed itself with its maximum of epidemic intensity among these new comers, who arrived either from departments where there had been no small-pox, so that consequently none of them had been attacked; or from departments where vaccination was practised with the least regularity. During an epidemic of small-pox the number of persons attacked causes the number of the susceptible to diminish every day, at the same time that the practice of vaccination, favoured by the fear of the illness, works in the same direction. It seems, at this time, that both the disease and the doctor conspire by different ways to arrive at the same prophylactic end. But, beyond this, I think that the human organism in time of epidemic small-pox can acquire a certain degree of immunity by being accustomed to the influence of the epidemic medium. This habituation at the small-pox centres has appeared to me remarkable, especially at the small-pox hospital opened at Bicêtre during the siege of Paris, the hospital to which I was physician in chief, and where were admitted, in five months, 8,000 cases of small-pox. In this hospital, where the wards as well as the atmosphere were impregnated with such a mass of morbid exhalations, we had not a single attack of small-pox amongst the forty doctors and dispensers attached to the establishment, in spite of the greater number of them neglecting to be re-vaccinated; neither was there one attack among the community of forty sisters of St. Vincent de Paul who nursed our patients night and day, and who lived in the centre of the hospital. This fact reminds us that in 1854, at Gallipoli, the regiments which stayed several months in the midst of the choleraic centre ceased to furnish fresh cases of disease, while the troops which arrived every day from France, and which had not the benefit of habituation to this infected source, suffered severely. There is, then, for small-pox, as

for cholera, a certain degree of protection conferred on the constitution by its prolonged stay in the morbific medium.

III.—In spite of the obscurity of the influence expressed by the term epidemic constitution (epidémicité) there is in small-pox especially one circumstance which proves in an incontestable manner the reality of its action. It is this. At all times, and especially in spring, small-pox remains inoculable almost in the same degree; except in the case of a previous attack, the constitution is not much more refractory to the inoculation of the fluid small-pox poison than to that of rabies or of syphilis; the susceptibility to inoculation is independent of epidemic conditions and by this means the virus is transmissible at all times and seasons. The contagion of small-pox seems, on the contrary, to lose all its power of atmospheric or miasmatic transmissibility in the interval of its epidemic prevalence. Again, let us take, for example, the small-pox hospital at Bicêtre: immediately after the war this hospital was restored to its usual purpose; many feeble old men and numerous employés re-entered this establishment, of which the atmosphere was certainly saturated with small-pox germs; none of these people were seized by the disease. The same thing was noticed over the whole extent of the city of Paris. Great numbers suddenly came to reside, and, nevertheless, in spite of the pabulum which these new arrivals offered, the disease did not renew its attacks.

What is, in such a case, the reason which prevents the propagation of illness by atmospheric agency? It is not a diminution of individual susceptibility, because this, except where a previous attack has been gone through, never becomes refractory to the endermic inoculation of small-pox. Certainly the disease would have been developed among a great number of the persons entering Paris or Bicêtre in 1871 if they had been inoculated with a lancet.

I think, then, that there are in the conditions of the atmosphere certain modifications, whether physical or chemical, which act on volatile contagia in extending or limiting their diffusion and activity. These modifications would be the principal cause of the epidemic oscillations of small-pox; and it is their influence, foreign and superior to the human organism, which would constitute epidémicité (the epidemic influence).

If we remember, moreover, that epidemics of small-pox are frequently accompanied, or immediately followed, by certain other epidemics, especially by epidemics of measles,

sometimes of diphtheria, or of erysipelas, we can suppose that the atmospheric conditions favourable to the diffusion and activity of the volatile germs of small-pox have the same influence on the germs of several other diseases, which show themselves especially on the skin and mucous membranes.

In conclusion, a last proof of the influence of epidémicité on small-pox is the temporary modification both in the form and the gravity of this affection, a modification which in the last epidemic was manifested by the abnormal frequence of hæmorrhagic small-pox, the cause of such variety certainly not being the individual constitution. It has been maintained, however, that the aggravation of the disease had been brought about by certain individual states, and that during the siege of Paris, for example, black small-pox had been the result of scurvy pre-existing among the patients. But this explanation cannot be maintained if we consider that black small-pox was especially common at the beginning of the siege, when there was but little privation, and if we bear in mind one very remarkable fact which I have cited ("La Variole, au point de vue épidémiologique", p. 122), which is that towards the end of the siege (in January, 1871), the moment when these privations had arrived at their maximum, some patients with scorbutus had small-pox, and, in spite of the received theory, their small-pox was mild, and in no way hæmorrhagic.

It has been maintained equally that black small-pox results from the massing together of patients attacked by the epidemic, and that if we bring together a great number of patients this alone is sufficient to excite an aggravation of their disease, a so-called hypervariolisation. It is enough to demonstrate that the mortality at Bicêtre has not been relatively more considerable than that of the smallest establishments where small-pox patients were admitted, to prove that these patients exercised little or no influence upon each other. Finally, let us remember that in many villages in France, where there were only three or four patients, some very far distant from each other, and under no mutual influence, black small-pox was, nevertheless, the prevailing form. Therefore it depended solely on the epidemic conditions of 1870.

IV.—This intervention of an influence superior to man, and consequently to the predisposition and susceptibility of the organism, is still more marked in the propagation of epidemics of cholera. It is enough to recite certain localities

which resist the development of this last disease in spite of the frequence and the extent of their communications, as in the case of Lyons, to prove that the transmissibility of cholera is subordinate to more complex influences than that of affections especially contagious, as small-pox.

The doctrine now so widely spread, of the parasitic origin and reproduction of epidemic affections, has led many distinguished observers to look for a vehicle by which, under a concrete and tangible form, these supposed parasitic germs may pass from the sick person to the person threatened with contagion. Researches of great interest made at London on the morbific influence of Thames water, the appearance of cholera in the English fleet during the Crimean war after taking in water from Baltschick, where the French army, while suffering with diarrhœa, had lately encamped, occasioned a considerable etiological value to be attached to the imbibition of water contaminated by choleraic products. Moreover, cholera is not the only affection of which the propagation has been sought in the fæcal contamination of drinking water. The same opinion has been started, founded on facts, both for dysentery and typhoid fever. These two diseases offer, as well as does cholera, two characters in common; first, the intestinal secretions are increased and altered; second, the intestinal tube is the seat of the morbid lesions; hence the mind easily yields itself to the idea of a propagation of disease dependent on an increased production of a materies morbi, and on its direct conveyance to that part of the body where the lesion characteristic of the disease is to be developed. I admit, for my part, the dangerous influence of contaminated water in the etiology of each of these affections, yet without considering this influence as specific. Drinking-water, impure either from products of simple putrescence, or from the excretions of persons, whether healthy or ill, seems to me rather a *common* than a *specific* cause of these affections. Bad water may produce a tendency to disease much in the same way, and be as little poisonous as errors in diet or exposure to cold, both of which may excite gastric irritation at the very time most important for the digestive functions to be maintained intact.

But I do not admit that the theory of contaminated drinking-water explains the march of cholera epidemics. Instead of descending the course of streams, cholera ascends them, as in 1873 from Havre to Paris, or proceeds at right angles to them, as in 1832 from Calais to Paris. In the

month of September, 1873, the epidemic broke out simultaneously in different quarters of Paris. It is evident that these places, remote from each other, attacked at the same moment, could not have been successively infected by the intermedium of water. Need the failure of the drinking-water theory to explain the propagation of cholera shake our faith in the effect of human communication in its transmission? I think not. Carefully investigated instances of insular localities attacked after infected arrivals afford the best proofs of this transmission. Quite close to the coast of France, on nine different occasions, we have ascertained the importation of cholera at different points of Algeria by ships proceeding from Marseilles and Toulon.

Only, here again, this transmissibility is subject to conditions superior to those which regulate human communication, and on these depend the generalisation and rapidity of progress of the different epidemics of cholera. Dr. Cunningham has made the interesting remark that, in spite of the present rapidity of travelling afforded by railways, the march of cholera was less rapid in India in 1872 than in 1817. We have been able to make a similar observation in Europe. Thus, cholera which broke out in Paris in 1832 came from Russia in less than two years, while now, in spite of our railways, the cholera which burst forth in Paris in 1873 had existed in Russia for nearly five years. Though these facts tend to show that the influence of human communication in the transmission of cholera is subordinated to an impulsive or restrictive epidemic influence, it is quite necessary to bear in mind that this latter power can never outstrip the rate of human communication, and is so far dependent upon our means of transit, that never until the introduction of steam navigation did cholera travel in eight days, as in 1865, from Alexandria to Ancona.

V.—These two affections, small-pox and cholera, have then some points of resemblance and of difference in the simultaneous influence exerted upon them by the two factors that we have had under our notice, viz: first, the human organisation (individual susceptibility), second, the epidemic constitution, or epidemic influence.

Viewed generally, epidemics of small-pox are slower in their development: they seize in succession on the different quarters in the same town, and in a garrison extend to different regiments one after the other. Whatever the topography or hygienic state of a locality, these epidemics linger long—perhaps for two or three years—and have a winter

recrudescence. Cholera, on the other hand, never prolongs its stay for long in one place, except in some rare localities where it finds conditions exceptionally favourable for its reproduction and permanent abode; everywhere else it sweeps past sooner than small-pox; it seizes at the same time distant quarters of the same town, and its recrudescence is in the summer.

It was thought that these two diseases, as epidemics, exercised some mutual power of exclusion one against the other, that cholera appeared rather at the time when bilious disorders prevailed, small-pox when other exanthemic affections were predominant. We have been able, in opposition to these beliefs, to substantiate the simultaneous existence of these two epidemics, in Russia in 1871, and even at the present time (1874) in certain towns in Germany.

In contradistinction to typhus, small-pox and cholera present the characters of not being intensified by bringing together a number of the sick, and of offering a prognosis almost identical, whether the cases are isolated or agglomerated. This question, so important with reference to the establishment of special hospitals, I have made the subject of particular study in my memoir on "The Influence of Hospital Establishments on the severity of Infectious and Contagious Diseases".

ON THE ALLEGED SPONTANEOUS PRODUCTION OF THE POISON OF ENTERIC FEVER.

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ALTHOUGH the symptoms of enteric fever had been described before very accurately, especially by French writers, it was reserved for Stewart, in 1840, to point out the difference between it and typhus fever and to establish the fact that they are two distinct diseases, and for Jenner and Murchison subsequently to show that the circumstances which favour their spread are essentially different.

But with regard to the origin and mode of propagation of enteric fever there are still considerable differences of opinion, some holding with Murchison that the poison of the disease may, and frequently does, originate spontaneously, and others with Budd that there is no proof that it ever does so.

Before discussing this point, it will be well for us to consider for a moment the characters of the diseases of the class to which it belongs. Trousseau says, and his opinion upon this point is very valuable, as that of a consummate clinician, "*Dothiénentérie* (the name given to this disease by Bretonneau) is a general disease, acute, febrile, very akin to the eruptive fevers of which we have spoken, and with which it offers more than one striking analogy." Let us see how far this "striking analogy" holds good. The diseases with which Trousseau here associates enteric fever, have certain remarkable characters in common, and form a well marked class of acute diseases; they are febrile; they have definite durations, which are divided into well defined periods, one of the most characteristic of which is the period of incubation; they have definite and characteristic lesions; they rarely attack the same person twice, and they are communicable from one person to another, in

other words they are contagious. Enteric or typhoid fever, undoubtedly possesses all these characters except the last, and now a word or two about that. Trousseau says, "*La contagion de la dothiénentérie est incontestable.*" Budd says, "This fever is an essentially contagious or self-propagating fever;" and, again, "as in small-pox so in typhoid, to spread by this mode of reproduction is not only a characteristic, but the *master*-fact in its history." Sir Thomas Watson is of the same opinion, and even Murchison, after quoting cases in which it is known to have spread from an infected person, is obliged to make the following remarkable admission, "With such facts before us it is impossible to deny that enteric fever is in some way communicable by the sick to persons in health," which seems to me to concede the whole point of the contagious nature of the disease; no one could admit as much of rheumatic fever or of ague, and that is precisely the difference between a contagious fever and a non-contagious one. We are then met with this kind of argument. "Well! we can't help admitting that the disease is under certain circumstances communicable from an infected person to a healthy one, but as we find a great many cases, indeed the majority, in which we are *unable to trace* any such connection, we must, therefore, assume that this is not the usual way in which it is propagated." But why should we make any such assumption? Why do we not make a similar one in the case of small-pox or of scarlet fever? It is because the method of communication is, as a rule at any rate, different; in these diseases the poison is given off into the air around the patient, and it is dangerous to approach him or to touch anything that he has used; while of enteric fever it may be said, as Mr. Simon says of cholera, that "the quality of infectiveness belongs particularly, if not exclusively, to the matters which the patient discharges by purging and vomiting from his intestinal canal." And which disease should we expect to be least obviously communicable? Clearly the one in which the poison is least diffused into the air around.

We are, however, told that the intestinal discharges from a patient suffering from enteric fever are not infectious when fresh, and only become so after decomposition has set in, and in proof of this it is advanced that medical attendants do not take it from the patients, that nurses do not get it, and that other patients in the same ward, although using the same utensils, scarcely ever take the disease. Now it must be admitted that such cases are uncommon, though

they are by no means unknown; indeed I find that while Murchison, on page 461 of the 2nd edition of his "Treatise on Continued Fevers," says, "I have never known nor heard of a case where the fever has been communicated to the medical attendant not residing in the infected house;" he says (p. 469) of Putegnat, who thought he had caught it from making a *post mortem* on a case, "It is true that he was seized a few days after the autopsy; *but he had attended both the patient and her mother during their illness;*" so that on one page we are told that the fever is not communicable because medical men do not get it from their patients, and ten pages farther on we find that the fact that a medical man got it after making an autopsy is considered to be no proof that the body was contagious, because he had been attending the patient and her mother during their illness.

But is it proved in any sense of the word that the fresh fæces of a patient do not contain the poison of the disease? Certainly not. All that can be deduced from the facts is that they do not give out the poison in any considerable quantities into the air, and this makes it probable, without reasoning from mere analogy, that the poison consists like the active principle of vaccinia, of solid particles, which do not evaporate from liquids in which they are, but may be carried up mechanically by the volumes of gas given off when such liquids ferment, or after the evaporation of the liquid may be dessicated and blown about; thus we see how it may be that the fæces have come to be regarded as more poisonous when decomposing than when fresh, or even as developing the poison during decomposition.

I say that there is not one iota of proof that the fresh stools do not contain the poison of the disease, and that to say so is like saying that a solution of sugar of lead or of corrosive sublimate, scented with, say, attar of roses, is not poisonous, because you can stay in the same room with it without being poisoned, or that a vessel of vaccine matter does not contain the active principle of vaccinia, because you can handle it without being affected by vaccinia.

I can understand the position taken up by Andral, who "declared that he had never seen it exhibit the slightest contagious character;" or by Dr. Stewart, who wrote in 1840, "In no case, though questioned with the greatest care, either in Scotland or in the hospitals of Paris, have I ever found the disease referred to contagion."

I can understand that taken up by Trousseau, by Dr. Budd, and by others, who maintain that the disease is

essentially contagious. But I cannot understand the anomalous, as it seems to me, position of those who in effect say, "Well it's not a contagious disease, but as you've certainly brought forward some cases in which it appears to have been communicated from man to man, we must admit that occasionally it is contagious."

That it is contagious, and most virulently so, I have not the slightest doubt; but I quite understand what those mean who say it is not; they mean that if you attend upon a patient suffering from enteric fever you are not likely to get it, while if you attend on one suffering from typhus or scarlet fever you are very likely to do so, unless you have had the disease before: they do not consider that this fact is not due to a difference in the contagious nature of the disease but to a difference in the form in which the poison is excreted from the patient, most of it being in the one case given out into the air which the attendants breathe, while in the other most of it is swamped in a mass of liquid which is removed as soon as possible. Those accustomed to small-pox, scarlet fever, and the like, of course said that enteric fever was not contagious when it was first brought to their notice, and there is no doubt that it is, under ordinary circumstances, very slightly so in their limited sense of the word; but that is not what is meant by those who now deny that, except under certain circumstances, it is not communicable from one person to another.

When we point to undoubted instances where a person suffering from enteric fever has taken it into a place where it was before unknown, but in which it has spread from that person, it is at once admitted "that there are unequivocal cases of enteric fever propagated in the manner described," but we are told that "such occurrences are exceptional, and that *the number of cases where the disease is introduced into a new locality without spreading far exceeds that in which it is propagated*," which means that the expedients adopted to get rid of the excretal matters were, in the cases observed, (probably chiefly in large towns), generally sufficient to ensure the immediate removal of the infected fæces from the premises in a short time.

But I would put the facts in another light, and would say—We see in how many cases we can prevent this disease from spreading; we can almost always point out the reason why it spreads in a house or town when it does so, and we ought to prevent its spreading much more frequently than we do.

That epidemics of this disease can often be traced to importation will hardly be doubted by any one; in one instance, which came under my own observation, I found that the first case occurred in a house the inhabitants of which were in the habit of visiting and receiving visits from relations in a cottage in a village at some distance where there were several cases of the disease; in another instance an epidemic, so severe as to cause a panic, occurred in a small town, and I ascertained that the first case was that of a young man who had been brought home ill, with a disease the nature of which was not at first understood, but which turned out to be enteric fever; and there are plenty of such instances on record. In large towns the difficulty of proving importation is often very great, and the difficulty of tracing the cases back to previous ones may be insurmountable; thus in one instance I was able to trace a severe epidemic of this disease to the chief inn of the town, but it was quite impossible to say who had brought it there in the first instance; again, in large towns the enteric fever poison spreads chiefly through the medium of sewer air, in which it is doubtless suspended, and in London I find that this fever especially prevails in houses where the waste pipe of the drinking water cistern communicates directly with the house-drain, a sanitary defect which is very common indeed in many parts of London, especially in old houses in the best neighbourhoods; to quote from my Report on the Sanitary Condition of the Parish of St. George, Hanover Square, for the year 1873:—"Out of 39 houses in which there were cases of enteric fever, 22 had the sanitary defect in question; while there were 11 other houses with the same defect, in which cases of low fever, or some other ill-defined disease, were found. It may be mentioned that diarrhœa frequently breaks out in such houses." The waste pipe being a ventilator, and often the only one to the drain, the foul air, which may contain, suspended in it, some of the poison of the disease, rises up this pipe into the space between the water in the cistern and the cover, and the suspended particles fall into the water which is drunk, and which may, and often does, give rise to enteric fever in the persons drinking it.

We see then that the poison of the disease may be taken from one place to another by a person suffering from it, may be communicated to other persons in that place, and may be carried about in sewer air; and what kind of evidence is adduced that the disease ever springs up in a place without the poison having been brought there?

Trousseau said that the poisons of all the contagious diseases might be developed at any time anywhere under suitable conditions, because they must have originated at some time, and in this way he placed small-pox, scarlet fever, and typhoid, all on the same footing; but this is a mere statement of opinion and not an argument, for there is no proof whatever that the poisons of these diseases ever were developed *de novo* in the sense that Trousseau meant; if the poison of small-pox or of enteric fever were a definite chemical substance like strychnia, or morphia, or prussic acid, we should expect that it would be formed under certain conditions, but if these poisons are living germs we should, on the contrary, expect them to flourish when implanted in a favourable soil, and as to their origin we should believe them to have been produced by modification of previously existing germs of a somewhat different kind, at any rate, until the possibility of the *de novo* origin of these living germs under suitable conditions, was clearly proved.

But as we do not really yet know what these poisons are, we must argue from facts of a different kind; and we are not *obliged*, in spite of their "striking analogy", to class small-pox and enteric fever together in this respect, for we may be told at once that Trousseau may be right about enteric fever, and wrong about small-pox; the poison of the former may originate spontaneously, and the poison of the latter may not; each disease must be studied by itself, especially as regards its ætiology.

Are we, then, able to trace every case of enteric fever to a previous case? Certainly not; and it would be very wonderful if we could! When we consider that a person suffering from this disease may go about for weeks, leaving the poison in several different places every day; that he may go about his work until so prostrate that he goes to bed to die; that he may fall down dead from perforation of the intestines without the disease having been recognised; that the poison which he has left in so many different places may be distributed broadcast in water, milk, sewer air, or some other vehicle—when we consider all these things, we may well wonder, not that we are often unable to trace the disease to infection from a previous case, but that we are so often able to do so. From the fact that we are able so frequently to point out the source whence the contagion has been derived, and to trace it to a previous case, we have at any rate a strong presumption in favour of the view that when isolated cases occur, and there appears to be no connection

with previous ones, the fact is that we are unable to trace any such connection, and it is clear from the nature of the disease that this must very frequently be the case; and, moreover, we have no right to assume that we even yet know all the methods by which the poison of this disease may be conveyed. It is only comparatively recently that milk has been recognised as a vehicle of contagion, and there may be, and probably are, many other media of which we have not yet any knowledge, by which the poison is conveyed from place to place.

It is clear that in towns, large and small, where strangers are continually coming and going, and where commodities of various kinds are brought from outside, and houses are connected with public sewers, it is not merely "difficult", as Dr. Murchison says, but on the face of it *impossible*, "to exclude the possibility of contagion", and so we may dismiss such cases at once. But then it must be allowed that many instances are on record where outbreaks of this disease have occurred in isolated villages and country houses, and where every effort has failed to trace any importation of the poison. It will be found on reading the published accounts of such cases that the possibility of importation in a variety of ways has been overlooked. A great many of them are cases of schools, or other places where a number of people are collected together. No number of such cases would be sufficient to warrant the assumption that the disease arose *de novo* in any one of them. In most of the other instances we find that old cesspools, stopped up drains, etc., have been disturbed, or have drained into wells, or leaked into the basements of houses. How are we justified in assuming anything more than that the filth contained the poison of the disease? How can we pretend for a moment to say that we have excluded the possibility of its introduction into masses of excrement which have been going on accumulating perhaps for years? The thing is manifestly impossible. And when we consider how many persons—visitors, servants, and vagrants—go to and from "isolated country houses", and their premises (stable-yards, etc.), we shall be disposed to hesitate before saying that we have excluded all possibility of importation.*

* A typical case has come under my notice since the above was written, and may be worth inserting. In an "isolated country house", where great attention had been paid to the sanitary arrangements, a solitary case of enteric fever occurred; out of a large number of servants only one fell ill, and no one in the part of the house where the family resided was attacked; the patient was removed to a cottage on the day before

I must confess that these considerations prevent my accepting any of the cases on record as cases where non-importation has been proved, and I cannot therefore accept the dictum that the disease "may be generated independently of a previous case by fermentation of fæcal, and perhaps other forms of organic matter". Were this true, would not the disease be much more prevalent? How is it that a house or town may be in a condition eminently suited for the existence of this fever, as shown by the fact that when a case is imported the disease spreads, and often becomes a severe epidemic, and yet no case is heard of there for many years, until the importation takes place? One town that I have already alluded to is a most filthy, overcrowded place; it has been several times decimated by cholera, but no enteric fever occurred there for many years, until one day a case was imported, and the disease spread like wildfire, showing that the conditions for its existence were most favourable; and such instances might easily be multiplied were it necessary. Besides, we know very well what disease sewer air causes—it causes diarrhœa. Over and over again have I seen whole

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the nature of the disease was certain, and no further cases occurred; a short time before this girl was seized, a drain had been opened under the windows of the servants' dormitories, and a very bad smell had been noticed; the girl who was attacked had been working rather hard, being "a willing horse", and had even refused to go home when permission was given her, consequently she had not been away for some time, and there is no doubt that she was in an enfeebled condition. I was assured that no cases had been heard of in the neighbourhood for miles around. The case, which I saw, was a severe one, and the typical *taches rosées lenticulaires* were noted. Here then was surely an instance of *de novo* origin of the disease. No cases anywhere near, a foul drain opened under dormitory windows, and one of the twelve female servants attacked—the one who was "out of sorts", and had been overworking herself. On further investigation, I found out by a fortunate accident that some cases had occurred some months previously at a single cottage in a village about six miles off; on visiting this cottage, I ascertained that a boy who worked out at some considerable distance in various places, had brought the disease to this cottage, and that his father and brother who both caught it and died of it, had worked as "rick makers" on the premises belonging to the house in question while they were ill, one of them working until he was quite prostrate, and went home to die; there can, therefore, be no doubt that one or both of these men planted the poison in the drain, and that it remained there for months, until it was disturbed by the opening up already mentioned; this probability was made almost a certainty by the additional discovery that the brother-in-law of one of the men (the one who worked longest) was actually employed at the time in one of the outbuildings of the premises, and was no doubt from time to time visited by the sick man. No doubt many such cases occur, in which the fortunate accident which results in the discovery of the connecting chain does not occur, and in which it may even be impossible to prove importation.

households prostrated with diarrhœa, when air from a foul sewer has leaked into the house, or air from a blocked-up sewer or soil-pipe has found its way into the drinking-water cistern through the waste pipe, and has fouled the water.

I therefore maintain that foul air contaminated by decomposing animal matters is capable of producing mere diarrhœa, and that when it produces enteric fever it contains the poison of that disease, and that the arguments adduced to prove that this poison can be generated from such decomposing matters, independently of a previous case of the disease, are inadequate to do so; that in many of the cases where non-importation is supposed to have been all but proved, it has not been even rendered a fair presumption, and therefore that in the present state of our knowledge we are not justified in saying that the disease ever arises *de novo*.

Finally, although it may at first sight appear that this is merely a theoretical question, and of little practical importance, such is by no means the case; the too general prevalence of the views held in this paper to be erroneous has led to great carelessness, not only on the part of the public generally, but also on that of medical men, in the adoption of measures for the prevention of the spread of the disease. When people are told that it is a disease which is only exceptionally communicable from one person to another they are not likely to try to prevent its spread, and they do not do so; the consequence is that infected stools are constantly thrown away without any disinfection, and thus the sewage of towns gets infected, and the disease spreads. It is true that the supporters of these views tell us that it is necessary to disinfect the stools, I suppose because, as Dr. Murchison says, "It is probable that the stools of enteric fever are more prone than ordinary sewage to the specific fermentation by which the poison is produced, and that this explains why the disease is occasionally propagated by the sick;" but in practice people will not do any more than they can help, and if they are told that the stools are not infectious, and that the disease does not generally spread, they will not take the trouble to use disinfectants, and thus I believe that much evil results. It might, perhaps, have been thought that the theory that enteric fever poison may be developed in any foul sewage would have made people more careful by making them afraid of all foul air and foul water, but this is not so; given foul air or foul water, it is impossible at present to tell whether or not it will produce the disease, as we cannot tell whether it contains the poison or not, and so,

under either theory, foul air and foul water are both dangerous. I think, therefore, that it cannot be insisted on too strongly that the disease is a communicable one, and is communicated especially by means of the patients' stools, which ought in all cases to be disinfected before being thrown away; and I hold that a theory which practically causes laxity in this direction ought to be strenuously opposed. If true, it will outlive the opposition, and no harm will be done by insisting on greater precautions; if false, as I believe it to be, and especially if mischievous too, the sooner it is abandoned the better.

INTRODUCTORY ADDRESS, SESSION 1874-75.

By the President, W. R. E. SMART, M.D., C.B.,
Inspector-General of Hospitals, Royal Navy.

GENTLEMEN,—Our society is to be congratulated on the series of papers which it had for discussion last session, from the able pens of Drs. Milroy, Tholozan, Léon Colin, Murray, and Cuningham, Professor Corfield, and Inspector-General Lawson, making it in that sense second to none in its annals. This is suggestive that our Association is a central point towards which communications on epidemic diseases are drawn, not merely from the furthest limits of the empire, but from beyond its bounds; and this may be taken as the strongest of reasons for its existence amongst medical scientific societies, and as a propitious one for its continuance.

At the first meeting of our last session I laid before you a summary of the chief epidemiological events of the past year, and I propose on this occasion to repeat a course which then received your approval.

After reciting the points most prominent in the history of the diffusion of cholera that began at Kiev in S.W. Russia in 1869, and in its alarmingly greater extension in 1873, I was able to state that at the beginning of November the disease was everywhere declining, having already disappeared from many of the principal coast towns on the Baltic, but lingered in Cronstadt, Riga, Revel, and Lubeck, and was not then extinct on the North Sea, in the ports of Bergen, Hamburg, Rotterdam, Antwerp, and Dort; nor in Havre and Caen in the Channel; while on the Mediterranean —Genoa and Naples, on the Adriatic—Fiume, and on the Black Sea—Sulina, remained in quarantine.

Of inland cities, St. Petersburg, Warsaw, Buda-Pesth, Vienna, Berlin, and Paris, still retained the infection when I addressed you. During November, the greater part of

these places were reported "clean and free from cholera", but it lingered in Rotterdam, from which a case was imported to Wapping on the 19th November, and died in the London Hospital.

In the middle of December, the Mediterranean and Black Sea ports were in the same position as those on the Atlantic; but it was said to have burst out amid the Spanish troops besieging Estella, in Navarre, near the Pyrennean frontier, early in December, ostensibly causing the raising of the siege, but this report should be accepted with caution, as nothing further was heard of it in Spain.

Later in December, after quiescence of several weeks, it reappeared in Munich, and through the winter most probably it was not extinct, as 9 deaths by it were officially announced for the last week in January, and 62 the first week in February, 52 the second, 30 the third, dropping to 9 in the last week of February, and to 2 in the first week of March, after which it rose again to 7, 10, 12, 20, the first week in April, after which it declined steadily 12, 12, 7, 1, 2, in successive weeks to that ending on the 9th of May, after which date there was no death registered. The climaxes were first week of February and the first week of April. Towards the end of the year isolated cases appeared in Berlin, and in March it was present at Stettin, its port on the Baltic, while on the North Sea shore, its last date was the 19th of January at Rotterdam.

It returned in March also in Prussian Silesia, destroying 388 people in Strelitz, Pless, and Gross districts by the 28th of April, and Silesia was not entirely clear before July, when cases were reported as occurring in Hungary, and also in Vienna early in August. On the 23rd of September an alarm was raised of a case in London. On the 23rd of September a case is said to have ended fatally in London, in the person of a foreigner, but whether sporadic, or after recent arrival from the continent, it does not appear.—"Lancet", October 3rd, p. 496.

The epidemic imported into Europe through Egypt in 1865, which revived in 1869 in Russia after extinction elsewhere in Europe, may have finally ended now after nine years' duration, completing the third pandemic cycle from 1865 to 1874; the first cycle having lasted from 1830 to 1837—seven years; and the second cycle fourteen years, from 1847 to 1861. The first cycle was continuous and devoid of any marked recrudescences, but the last two cycles have been distinguished by such, attended by invasive

force, from provinces where it had never been thoroughly extinct, into others where the primary invasion from Asia had died out.

These facts are of importance, proving that although epidemic cholera has not yet evinced a liability to become endemic in Europe, yet it is capable of lingering in its sporadic form (query, as cholera nostras), and of thus retaining latent specific energy to renew its epidemic type under favouring conditions of people, place, and season, in degree and form equally destructive with those characterising its primary invasions from India.

The present year has afforded epidemiologists unquestionable data with regard to these recrudescences in Russia from the able pens of Dr. Pelikan and Dr. Archangelsky, making them guiding points for future action.

The former has shown from official reports that in countries lying to the east and to the south of the Elbe, including Russia and Poland, the Principalities, Hungary, Galicia, Prussia, and Poland, there have been since 1869 not less than 902,706 attacks and 371,572 deaths, the relative mortality amounting to 41.6 per cent. In the cities of Warsaw, Vienna, Munich, Berlin, Venice, Trieste, and Rotterdam, exclusive of those of Russia, there have been, as far as my data inform me, 14,649 attacks, with 6,448 deaths —a relative mortality of 44 per cent.

Great as these results of a secondary, and, as we may believe, a preventable, epidemic have been, they fall far under those of the diffusion that took place from Bohemia in 1852, lasting until 1861, when, according to Dr. Archangelsky, there were in Russia and Poland alone 800,892 attacks and 352,263 deaths; showing a percentage mortality of 43.98, or about 44 per cent.

Such ample statistics involve a conclusion that epidemic cholera, when re-vitalised in European nests, retains its highest degree of mortality, as well as its primary invasive energy. These secondary outbursts and diffusions of cholera seem to depend on local or on regional conditions that favour the resting of the sporadic, and the re-development of the epidemic type, with subsequent diffusion under favouring causes.

The closest examination of the regional conditions will be of vital importance; and together with it there should be a comparative one into the conditions of those countries where immunity has been enjoyed completely or relatively, as in the late epidemic that has occurred in England, Denmark, France,

and the Rhenish provinces. For, if unquestionable reasons can be assigned why the disease lingers in sporadic type in some localities, and then assuming the epidemic type, diffuses itself widely, failing to infect some other countries, we shall then be in a better position to stamp out the embers of a primary invasion, and to deprive a cycle of the disease of at least one-half its mortality—the first step towards excluding it altogether from Europe.

Successive International Congresses have busied themselves exclusively to devise measures for barring out the primary invasions from Asia, and have dwelt less on the equally important question of quenching its flickering embers; although the first may be unattainable, owing to the numerous frontier channels through which the disease may be introduced in the wake of commerce, while the second is attainable, by the concurrence of nations in sound views of the circumstances under which secondary foci are formed, and by decided action in those localities where the evidence of its existence in the sporadic type still lingers.

Whether the first infection reaches Europe stealthily by Southern Russia, the danger of which has been ably pointed out by Mr. Radcliffe, or more rapidly through Egypt, of which the future probability is greatly diminished, owing to newly devised sanitary measures in Hedjaz, and control of maritime commerce through the Suez Canal, its primary epidemic force will be regulated by the degree of congeniality of the nests into which its germs are conveyed, when the entire range of fostering influences would appear to be in prime degree under control, our own municipal history bearing strong evidence to it, and we have the highest interest in the dissemination of our experience.

By our scientific investigations into the causes and the diffusion of the disease in India, we have done more than all other nations put together to arrive at rational conclusions that will tend to strangle the disease in its cradle, and at home we have learned that, by sanitary measures, the virulence of the germ may be modified and the epidemic intensity may be limited, and the formation of foci prevented.

In Western Europe, except the Peninsula, there is a general coincidence with our insular experience, and a growing confidence in the superiority of a reasonable detention under observation to any prolonged quarantine with a view to exclusion of the germs of the disease, every reasonable precaution being exerted, and the general tendency is in favour of cleanliness of persons and of towns, disinfection and removal of excreta, and uncontaminated water supply.

In Eastern Europe these modern hygienic ideas are not equally trusted, for in all things our ready adoption of the unknown and problematical is the converse to our guidance by the known and physically certain. This is of so much the greater importance to ourselves in that, on every occasion except that of 1865, the cholera has come to us *viâ* Russia, and therefore we must feel the utmost desire for the enlightenment of its officials.

At the commencement of last session I dwelt on the wide spread of yellow fever in the New Continent, from Monte Video to the North American states bordering on the Gulf of Mexico, where it appeared at 250 miles from the coast, which rendered it difficult to trace any connection between the infected regions. It became extinct in the United States on the setting in of the cold of winter. At a later date it made itself slightly felt in Jamaica, at Kingston and Port Royal, but it became extinct there also at the close of the year.

It may be doubted that it ever became extinct in South America, although quarantine ceased in Brazil at the end of August. It reappeared in Brazil, at Para, on the 23rd of January, 1874, about the climax of the hot damp season, and later at almost all the ports south of it to Monte Video, in which it had been present in 1873; but the visitation has been of a less virulent character, its widest spread having existed in July and August. At the end of that month Brazil and the cities on the River Plate were pronounced free from the disease.

In August and September it re-visited the northern coast of the Gulf of Mexico, from Galveston in Texas to Darien in Georgia, including New Orleans, where it was unfelt in 1873, and cases of it were introduced from the Gulf into the quarantine stations of Baltimore and New York; but it was officially reported at the middle of September that these outbreaks furnished no occasion for any serious apprehensions that the fever would become epidemic, from which it may be inferred that it differed much from the visitation of the preceding year.

About Christmas, 1873, very alarming accounts reached England concerning an outbreak of yellow fever in the Bights of Benin, seeming portentous against the success of the expedition against Ashantee, but, happily, that danger was warded off by rigid quarantine practised at Cape Coast Castle, which restricted the disease to the delta of the Niger.

To Dr. Walter Reid, R.N., who did the duty of health

officer afloat, I am indebted for some interesting data concerning it.

The first knowledge at Cape Coast Castle of the presence of yellow fever in the Bights was late in November, on the arrival of the mail packet Ambriz, having the disease among her crew. She was at once quarantined, her mails being put on board the Biafra, and both vessels steamed off. The Ambriz lost thirteen of her crew before she reached the Cape Verds, and the Biafra six. The disease had broken out early in October in the palm-oil district, spreading eastward to Gaboon, but not westward of Benin 500 miles east of Cape Coast Castle; Accra, the port for Glover's contingent, lying between Cape Coast and Benin. Quarantine was enforced on all arrivals from between Badagry, west, and Cape Lopez, eastward. At that season the wind prevails strongly from the S.W., so that steamers only, and these are mail packets, could come from the infected district to the seat of war, of which six arrived at their appointed dates. They anchored two and-a-half miles leeward of the Castle, and were excluded from all intercourse with the shore, the mails only being landed from them and disinfected under naval supervision, and on receipt of the homeward mail-bags they departed after a few hours' stay. Similar regulations were enforced at Accra, and thus the forces were protected from infection.

From inquiries into the sources of the outbreak on the Niger, Dr. Reid doubts that yellow fever is ever extinct there; but he states that, in the summer, one of the Cape Verds—Sal—was under strict quarantine, with the rest of the group, on account of an epidemic of the disease. Small vessels laden with grain trade between the Cape Verds and the Bights, and some of these proceed at first from the Brazils, of which Dr. R. boarded one off Cape Coast, the Brazils being then infected.* From this it may be conjectured that the disease was carried by such vessels to the Cape Verds, and onwards to the ports on the delta.

It does not appear that any case of yellow fever displayed itself either in the vessels of war or among the land forces; and yet there was suspicion that H.M.S. Amethyst took the disease from Cape Coast to Ascension.

* Léon Colin "Gaz. Hebdomadaire," p. 59, Ann. Vol., 1874. The coast of the Gulf of Mexico, the West Indies, in Africa, the coast of Guinea, are the regions where yellow fever is endemic, which may be styled the homes of yellow fever—"From the mouths of the Niger to those of the Congo."

This war steamer was despatched from the coast with a large number of invalids, many of whom were from Elmina. After five or six days at sea, she reached Ascension on the 20th December, and landed her invalids. Among these there was one of continued fever with great nervous tremor, who died on the seventh day from landing. Another was moribund when sent ashore, dying in thirty-six hours. The symptoms were great anxiety, intense headache with deafness, epigastric pains, dry red cylindrical tongue, rapid pulse and respiration, injected eyes, a yellow surface, ecchymosed as death approached, relaxed bowels, vomiting of dark tar-like fluid, and suppression of urine—symptoms closely allied to those of yellow fever.

On the third day after being landed, one of the invalids, after a few hours of slight discomfort, fell down almost lifeless, and vomited and purged at the same moment a dark tar-like fluid mixed with pure blood; the features were deathlike, the surface cold and of a dirty yellow colour, pulse scarcely perceptible, respiration slow and sighing. Under treatment by stimulants, with opiates, he rallied, the vomiting ceasing after a few hours, when a typhous state supervened, lasting to the end of the second day, after which he convalesced.

Another case was similarly attacked on the fourth day after landing, went through an analogous course of symptoms, convalescing from the end of the following day. The train of symptoms and events in both cases were sufficiently diagnostic of severe bilious remittent fever of our English nosology, and of the gastro-enteric type of algide pernicious fever of the French nosology; from which we may infer that the disease carried from Cape Coast to Ascension in the Amethyst was of the remittent, and not of the yellow fever type. The sudden attacks of collapse, with almost immediate vomiting of tarry fluid mixed with pure blood, with early reaction ending in convalescence, were the characteristics of these cases, and these are pathognomonic of fever from paludal miasms. In yellow fever such accessions are most rare, and vomiting, as described, is seen only after the first stage has passed away, while such rapid convalescence from yellow fever, when it has once set in, is an unheard-of event. Therefore, these cases are proofs that the most terrific aspect of fever in the expedition was not of yellow fever type; and, taking in review the history of the time and place, strong evidence is afforded of the efficiency of quarantine in protecting highly predisposed masses from that fearful malady.

The season for its appearance in the West Indies is with the first drying of the soil after the autumnal rains, and these having ceased in Jamaica early in October, fever became prevalent at Kingston early in that month.

The naval station at Port Royal is on a sandy spit four miles distant from Kingston, eight miles across the bay, where lay H.M.S. Aboukir, a wooden line-of-battle ship, whose holds and bilges were foul from leakages, which had received on 13th October from England a large increase of men in vigorous health, and therefore the more susceptible of the germs of this disease. These, together with the Aboukir's own crew, were allowed to visit Kingston on leave, and to sleep there, and this source of danger was uninterrupted until the date of the first case in the Aboukir.

On the 26th of October the first case occurred in that ship, in the person of a man who had slept in Kingston on the night of the 20th of October, after an incubation of six days.

In this manner a focus was developed in the ship amid her local insanitary conditions. From that date up to the 12th of November there was a daily succession of cases of simple continued fevers sent from her to the hospital, with one case of yellow fever on the 4th November, eight days after the first case, and another on the 5th, from which date twenty-six cases of yellow fever were sent from her, the climax lying between the 25th November and 12th of December.

Up to the middle of November some of her crew went daily to work in the dockyard, and several of these were virulently attacked.

A second focus was formed in the dockyard, and a boat's crew of eight men stationed there suffered severely between the 5th and 23rd of November, four of them dying of yellow fever, two others undergoing severe forms of continued fever without black vomit and suppression of urine, all contracted in that focus on shore. The ostensible seat of this new focus was in the sleeping apartment of these men, adjacent to which there lay a new drain or sewer, 200 feet in length, with a fall of only two feet to the sea, and its mouth open to the wind at certain times of the tide. The disease ceased among the boat's crew on locating the fresh men in another building, but it is significant that two deaths in officers' families occurred between the 22nd of November and 6th December in houses connected with that sewer. In the hospital itself only one case originated,

it being difficult to segregate perfectly the yellow fever convalescents from others; and this case, although severe, recovered, he being at the time of attack under mercurial treatment.

From the 13th to the 20th of December there was no fresh attack in the Aboukir; but there was then a reappearance of the disease in one case, and on the 25th in two cases, of virulent form. This led to the removal of the crew into other ships of war, which carried them to Bermuda and Halifax.* While they were absent the Aboukir was cleansed, and a leak was discovered that had rotted a portion of her timbers and carried into the bilges a white slimy fœtid matter from the decaying wood. Very ingenious means were employed to correct this defect, and since the return of the crew to the ship no other outbreak has occurred.

It has often happened that in ships stricken with yellow fever there has been found on clearing-out some equally probable cause of the infectious focus formed in her; and it may be said that in the United States scarcely any other opinion prevails than that yellow fever is generated where unsanitary conditions prevail, and that the infection is one of place rather than of person: that while it is dangerous to remain in ships thus infected, their sick may be received safely into roomy well-ventilated hospitals.

The Plague has reappeared in 1874 at three far remote points—in Kurdistan and Mesopotamia, in Western Arabia, and in the ancient Cyreniaca—which points had been severally the last seats visited by it prior to the present outbursts in them in this year. Thus in 1858 it was prevalent at Benghazi in Cyreniaca, in 1867 in Mesopotamia, and in Kurdistan in 1871; Egypt having been spared on each of these occasions, although formerly a nidus during its stage of poverty; but now, rising in prosperity through its newly developed commerce, it is free from plague.

In all these outbursts of the disease, from 1858 to 1874, want, often approaching to destitution, has been the forerunner of its development.

This year it appeared in February amidst the Arab tribes of the Dazarah, in the district of Hillah, near to Bagdad, where it was present in 1867; early in the year also at Bana in Kurdistan, where it was in 1871; and in May at

* In one of the ships—the Sphynx—the crew contracted the fever from her passengers, and of six attacks, four died. The other two ships had no manifestation of the disease in their crews.

Merj in the Cyreniaca, extending to Benghazi, where it was felt in 1858.*

The cause of its appearance in the same year, at geographical points so widely separated from each other, may still remain amongst the unexplained facts in the history of endemo-epidemic diseases. The coincidence is one of importance, and it is worthy of deep consideration with reference to our late president, Inspector-General Lawson's theory of pandemic waves, that the infected districts lie within two of the isoclinal lines of latitude marked on the map, to define the annual existence and progress of diseases of this class.

Europe is greatly indebted to the Turkish and Egyptian governments for their prompt and vigorous efforts to limit the disease to the seats of its outburst, by scientific inquiry leading to a definite knowledge of its nature, and by preventive measures. Destitution is proved to have preceded it. By the last accounts from Benghazi it appears to be general in that province.

Re-surveying the ground we have passed over :—

Cholera has been seen to re-vitalize from its almost extinct embers, in situations which are under the most primitive conditions of any in Europe, as regards the habits, social status, and sanitary precautions of their populations.

Yellow fever has shown itself in the United States, at a point far inland from the Gulf of Mexico, and has spread from thence to the Atlantic shore; dying out with the advent of last winter, it has reappeared this year along the coast of the Gulf. At its first outbreak at Shreveport it was attributed to obstructed sewerage of the town, which has not been, so far as I am aware, disproved.

In South America, also, the epidemic has been felt at all the coast, from Cayenne to Buenos Ayres.

Plague has broken out in Africa, at spots signalised by having been the scenes of its last displays. Egypt and the Levant were formerly its nests and abiding-places ; but now they remain untouched by it, driven back, as it is, to the wilderness, among Arab tribes in the marshy rice-growing plains of Mesopotamia, or those of Africa bordering on the Libyan deserts, where men dwell in rock-hewn caves, and

* "Times"—Gaz., 4th Nov. date (printed 7th November), "Board of Trade: Madrid—In consequence of the malady which has appeared in Arabia not being Levantine plague, all arrivals from that country will be considered clean."

in both positions are subjected to not unfrequent periods of want.

These three scourges of mankind have broken out where very low sanitary conditions have prevailed, at least as concomitants, if not as actual producing causes. Towards them the sanitary engineer, guided by medical science, stands as the modern Hercules.

We are not in a position yet to question the theory of specific causes or germs of those diseases, but as our knowledge advances regarding their developing and centralizing influences, we shall arrive at a clearer idea whether these germs are capable of being produced *de novo,* and if so, under what circumstances they may be generated.

Although it took two hundred years of ever progressive civilisation to sweep the plague from Europe, beginning with that unsought disinfector, the Great Fire of London, yet we may believe that in our age the light of sanitary science, already productive of obvious results, in London especially, will shed its rays farther and farther into the recesses where cholera haunts, and drive it ultimately, like the plague, out of Europe in a much briefer space of time.

From those distant dates when the causes of our "black assizes" were explored in the dungeons of our county prisons, and in the places of confinement of felons throughout Europe, by the immortal John Howard, Dr. James Lind, following his teaching, showed that the so-called putrid fevers of fleets, and Dr. Pringle extended the investigation successfully to the contagious fevers of armies and camps, our nation has ever been foremost in applying sanitary preventive measures to contend against those diseases, in preference to measures of rigid quarantine.

These principles are still, and ever will be, in the stage of growth, but in late years that growth has been rapid; and, as we have seen, they are extending more and more widely, dispelling the clouds of ignorance and fatalism that have surrounded the question in the minds of individuals and communities. Let us but act with greater energy, in the belief that this is one of our missions among mankind, and we shall find more speedy results to our philanthropic endeavours.

Before closing, I would add a few words concerning our domestic epidemics of the minor zymotic diseases, during this period.

The small-pox lost epidemic proportion in 1873, but in 1874 it has existed in less degree in the great manufactur-

ing centres, Birmingham and Bradford, West Riding, and Gloucester; scarlet fever prevailing in London, Lancashire, West Riding, Warwickshire, and Durham. Scarlet fever has been *the* epidemic of 1874, existing largely in Sheffield and Leeds, and other northern towns, but now, happily, declining there. In London, where it is never extinct, it spread greatly at the east end in May and June, and became epidemic in August, but still exists in the north, east, and south districts. Enteric fever has increased, *pari passu*, with it, but measles has declined. In combating these diffusions London has found the immense advantages of the special hospitals constructed in 1870-71 for the reception of small-pox, which are now availed of for whatever zymotic diseases may temporarily prevail.* With these at their disposal, officers of public health have at hand the means of early isolation of the most diffusive of this entire class of disease, whereby we may count on the limitation of infection, and the cutting short of those epidemics; thus reducing further the bills of mortality of the greatest, and even now the most healthy, city in the world.

This is the great exemplar of the results of sanitary science; and we may trust to see its teachings soon extended, with like results, to all our large cities and towns.

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* Registrar General's Weekly Report, dated 2nd November. Scarlet fever, death-rate annual: 2.0 in Salford, 2.4 Sheffield, 3.0 Birmingham, 5.4 Bradford, 8.3 Liverpool, and 2.0 London. Small-pox continues fatally prevalent in Birmingham, and enteric fever in Hull.

In London: deaths from zymotic diseases considerably below the average, excepting scarlet fever, from which, in the three previous weeks, there had been registered 107, 118, and 105, rose in last week, to 31st October, to 128, a higher number than in any week since 1870; 48 were returned in the east, 30 in the north, and 26 in the south group of districts.

ON RECENT OUTBREAKS OF PLAGUE.

By E. D. DICKSON, M.D.,
Physician to the British Embassy, Constantinople.

[*Read December 9th*, 1874].

On April 11th, 1874, Mr. Paduan, Inspector of the Health Department at Bagdad, telegraphed to the Board of Health at Constantinople, that "a bubonic malady had manifested itself amongst the tribes inhabiting Dughara, in the district of Hillé, near Bagdad;" and on the 23rd April he communicated to the Board the following statement, made by Rassam, Sheik of Dughara.

"A few days before the feast of Coorban-Baïram (January 27th), one of my tribe, named Safah, went on business to Afidj—six hours distant—and passed the night with the tribe of Berekat. Next day he returned home, and he and his wife fell ill, and died after twelve hours' illness. Their children, a son and a daughter, died also, after forty-eight hours' sickness, marked by vomiting of black matters. Safah's nephew, who had arrived from Afidj, washed the four bodies, and conveyed them to Nedjeff-Ali, to bury them there. On his return he fell ill; after which, his wife, son, and two daughters also fell ill. They all died within a few hours of each other. One of their relatives, Setade, who, together with his wife, had washed these five corpses, got the black vomit, and died in a few hours, as well as their infant daughter. Hussein, Setade's father, was also seized with the black vomit, and died in a few hours. A female neighbour, named Pelha, who was in communication with the afore-named family, was taken ill with a like vomit, and died in a short time. Her four sons underwent the same fate at intervals of a few hours one from the other; and all of them vomited *black blood*. One of Pelha's brothers having heard what had befallen to his relatives, went to assist them, washed the corpses and conveyed them to Nedjeff-Ali, where he died in the house of Mollah Nahir-Gazali.

"After these accidents, the malady declared itself in the neighbouring villages, and the mortality from it increased day by day, until it reached more than twenty deaths *per diem*. Besides the black vomit, these patients had diarrhœa, headache, fever, swellings in the parotid, axillary, or inguinal regions, varying in hue from a pale red to black; and after death, black spots were observed on the skin. The greater part of the bodies were transferred to Nedjeff-Ali, for burial."

To this statement, Mr. Paduan added the declaration made by Suleiman, a soldier belonging to the irregular troops stationed at Dughara, who deposed as follows:—

"In the beginning of the month of Sefer, I was seized with chills, intense fever, headache, thirst, darting pains in the left groin, followed by a swelling, which suppurated and burst on the seventh day, and which took twelve days to heal."

On the 16th April, a commission, composed of the physicians attached to the sixth army corps, together with Mr. Paduan, went to Dughara, and ascertained that the malady reigning there was similar to that which had appeared at Hindiyeh in 1867—characterised by black vomiting, diarrhœa, headache, shooting pains in the groin, axilla, or submaxillary region, followed by swellings in those places. After death, petechiæ showed themselves, but no carbuncles were noticed. The commission was told that the malady had declared itself a few days previous to the Coorban-Baïram, and had lasted until the present moment. About five hundred attacks had occurred; barely one-fourth of which had recovered. The commission found the inhabitants of this district living in filthy habitations, surrounded by rice fields and stagnant water. The greater part of the dead have been conveyed to Nedjeff-Ali, for burial. The malady (continues Mr. Paduan) at present appears to be declining; for most of the cases now get well, whereas at the commencement all attacked by it died after a few hours' illness.

At this juncture (says Mr. Paduan) firing commenced between the people of Dughara and those of Afidj, which compelled the commission to withdraw in haste, so as to avoid getting implicated in the disturbance; for these tribes are constantly at war with one another, and always hostile to the government. Subsequent telegrams from Mr. Paduan have informed the Central Board of Health that a military cordon has been placed round the affected country, and

strict quarantine imposed on arrivals from it; they, moreover, add that the malady has declared itself as well at Divaniyeh, Afidj, Milhatia, Hillé, Nedjeff, and at Elgiat. Despatches from the English and French consuls at Bagdad addressed to their respective embassies at Constantinople, amply confirm these details.

Later, another commission, composed of Mr. Paduan and Drs. Adler, Hixfal, and Hampierre, proceeded to Divaniyeh, and from that place telegraphed to the Board of Health, on May 16th, the following statement of the cases examined by them :—

Youssouf, aged 26 years : five days ill with intermittent fever; vomiting, diarrhœa, headache, and swelling in the left groin.

Abash, aged 22 years: three days ill with fever; headache, tongue dry and very black, thirst, vomiting, diarrhœa, swelling in the left groin as large as one's fist, and carbuncle on the left foot.

Hamondi, aged 16 years: two days ill with bilious vomiting; swelling in the left groin, fever, and carbuncles on the feet.

Djevat, aged 16 years: two days ill with profuse perspiration and fever, and swelling of the left parotid.

Mehmed : three days ill with violent fever; ardent thirst, diarrhœa, and swelling in the left axilla.

Hadjeïn, aged 10 years: two days ill with fever, diarrhœa, and vomiting. Deafness, moreover, supervened on the eleventh day.

Youssouf, aged 29 years: four days ill with fever; vomiting, diarrhœa, eyes injected, and swelling in the left axilla.

Hassan, aged 30 years: one day ill with fever; bilious vomiting, and swelling in the left groin. Died two hours after having been seen by the commission; and petechiæ appeared on his body.

Abdi, aged 19 years: four days ill with violent fever; prostration, tongue covered with a white fur, and swelling in the right groin.

Keani, aged 15 years: three days ill with fever; bilious vomiting, and swelling in the right axilla.

Hizir, aged 32 years : five days ill with violent headache, fever, blindness, and swelling of the parotid.

Azizieh, aged 24 years: five days ill with fever, and swelling in the right axilla.

Hassan, aged 27 years: four days ill with fever; vomiting, diarrhœa, and swelling in the left axilla.

Esma, aged 14 years: seven days ill with fever, and swelling in the left groin.

Hulvan, aged 24 years: three days ill with fever; bilious vomiting, diarrhœa, swelling in the left groin, and a carbuncle a little below it, prostration.

Ali, aged 7 years: fever, vomiting, prostration, and swelling in the left groin.

Hitchous, aged 18 years: living in the same house as Ali; thirty-five days ill with fever, vomiting, and swelling in the right axilla.

A gendarme (policeman), aged 27 years: three days ill with violent fever; vomiting, injected eyes, cold perspiration, and swelling in the right axilla. Died on the day he was visited by the commission, during which, carbuncles also appeared.

Selim, aged 35 years: nine days ill with fever; headache, tongue dry and black, swellings in the axilla and in both groins, carbuncle, and prostration.

Hadjel, aged 15 years: five days ill with fever; ardent thirst, headache, swelling in the right axilla, and two carbuncles on the chest.

Temr, aged 10 years: four days ill with fever; ardent thirst, vomiting, prostration, and swelling in the right axilla.

Shemil, aged 25 years: three days ill with fever; diarrhœa, vomiting, and swelling on the back of the neck.

Merir, aged 28 years: two hours ill with slight fever; ardent thirst, headache, and swelling in the left groin.

Fatma, aged 16 years: four days ill with fever; ardent thirst, copious vomiting, diarrhœa, headache, swelling in the right axilla, inflammation of the throat, and swelling of the parotid.

Hussein, aged 2 years: four days ill with violent fever; vomited once only, prostration, and swelling in the left groin.

Halil, aged 6 years: four days ill with fever; vomiting, and swelling in the left groin.

Fatma, aged 5 years: one day ill with fever; vomiting, tongue red, and swelling in the left axilla.

Hasheb, aged 28 years: four days ill with fever; ardent thirst, vomiting, diarrhœa, tongue black, and carbuncle on the left cheek.

Hakki, aged 4 years; fourteen days ill with fever; ardent thirst, tongue black, diarrhœa, and swelling of the right parotid.

On some of the dead bodies examined by the commission petechiæ were seen; on others not. The symptoms presented by these cases will leave no doubt as to the malady being the plague. It results from the above twenty-nine cases recorded by the commission that only four were females; and as these are seldom shown in Mussulman countries, except when very young, no inference can be drawn with respect to the proportion in which the two sexes were affected by the epidemic. The age of these twenty-nine cases varied from two to thirty-six years. All had fever; but in one instance it was of the intermittent type. Eighteen cases were affected with vomiting; eleven with diarrhœa; and seven with headache. All had swellings (buboes)—eleven in the groin; thirteen in the armpit; four in the parotid; and one on the nuclea. Seven cases showed carbuncles; and one or two instances exhibited petechiæ after death. The duration of the malady, at the time of the commissioners' inspection (with the exception of one case—thirty-five days old), varied from a few hours to nine days.

A report has lately been spread that plague had also manifested itself at Sinna, in Persian Koordistan, not far from Bana, where it had appeared in 1871; this report, however, requires confirmation. On the other hand, the plague has shown itself at Merdj, a town in the district of Barca, twenty hours distant from Bengazi, a seaport on the north coast of Africa. Restrictive measures have been applied in this instance also; and two medical officers have been sent to the infected locality to inquire and report on the outbreak.

It is worthy of remark that this malady has reappeared in the very same places where it had manifested itself on a previous occasion, viz., between Bengazi and Derna, in 1858; at Hindiyeh, in Mesopotamia, in 1867; and at Bana, in Persian Koordistan, in 1871. All these outbreaks appear to have been preceded by severe privations amongst the inhabitants—famine caused by drought or by inundation.

ON THE RELATIONS BETWEEN DIPHTHERIA AND SCARLATINA.

By ARTHUR RANSOME, M.D., M.A. (Cantab.),
Lecturer on Public Health, Owens College, Manchester.

[*Read January 13th*, 1875.]

THE following case shows the close connection between these two diseases :—

On February 23rd, 1873, scarlet fever attacked one of the boarders at a large public school. No cause of infection was discovered, but the boy was at once removed to the sanatorium. He returned to school on March 25th, and on that day two other cases commenced; and on the 29th, 30th, and 31st, others were attacked. These cases show the general outbreak of scarlet fever. On April 2nd, one of the masters, A. B., was seized with a rigor, followed by feverish symptoms and a sore throat. He came home to Bowdon at once, and on April 3rd, when I saw him first, there was no rash, either on the body or on the throat; but the fauces were much swollen, and there was general pyrexia; pulse 120; temperature 103 deg. On the 4th, there were diphtheritic patches almost covering each tonsil, and there was profuse muco-purulent discharge from the nostrils, the pulse still 120, the temperature 104 deg. On the 5th the fever had diminished, the temperature being 102 deg., but much albumen was found in the urine, and the throat and nose were still much congested, and the mucous membrane partially covered with patches of exudation. On April 8th, the throat was well and the albumen had disappeared from the urine; from this time he steadily improved, and was convalescent on the 14th. But on April 9th, six days after his arrival at home, his mother was attacked by the disorder; the temperature reaching 101.5 deg., and membranous exudation appearing on both tonsils; but there was no rash, and no albumen appeared in the urine. On April 14th, she also was convalescent.

The origin of these cases must surely be traced to the epidemic of scarlet fever at the school, as there were no cases of diphtheria either in the neighbourhood of the school, or near their home.

The close relation between these diseases has often been pointed out.

In the report of the medical officer to the Privy Council for 1859, Dr. B. Sanderson reports two instances in which pure diphtheria co-existed with pure scarlatina, and one in which scarlatina followed diphtheria: and again, in another place, three cases of diphtheria followed three of scarlet fever, and the former spread to a neighbouring cottage.

Again, Dr. N. Radcliffe remarks that diphtheria first became truly epidemic throughout England in the years 1858 and 1859, at a time when scarlet fever was raging; and it increased in prevalence, whilst the scarlet fever declined.

These diseases have also much in common. They are alike in their zymotic and epidemic character: both are characterised by high pyrexia; their chief point of attack is the throat and the glands of the neck, and a scarlet rash is found in both diseases. The sequelæ of the two disorders are also strikingly similar: suppuration in the lymphatic glands, ulcerations of the ears, arthritic affections, with or without cardiac complication, kidney disease and dropsy, general or localized paralysis of the nervous system. I have seen each and all of these consequences after both diseases.

It is probably in consequence of these points of resemblance that some physicians have regarded them as the same disease; the same poison producing different manifestations: but, in spite of their similarity, I venture to think that the differences between them are too important and too numerous to permit of so simple a solution of the problem. In the first place, although, as this case shows, diphtheria may apparently arise directly from scarlet fever, I have never known the converse action: I have never seen scarlet fever communicated by diphtheria; and some doubt as to the direct origin in diphtheria of the case of scarlatina recorded by Dr. Sanderson may reasonably be entertained.

There are also several points relating to the mode of origin of these disorders in which a difference is manifest. Thus, although both are contagious, diphtheria is undoubtedly much less so than scarlatina. In many instances, the cases are single—no contagion spreading from them to the many individuals exposed to their influence; and in others, where several cases have occurred in one house, it has ap-

peared probable that their seizure was due rather to a common cause than to the spreading of the disease from one to another. The usual mode in which scarlet fever spreads is certainly by contagion, and it can travel in clothes or other fomites, and may be transmitted by nurses or other attendants on the sick; it may even be conveyed by means of milk. I am not aware that diphtheria has ever spread by any of these means.

The two diseases differ in their geographical distribution. Thus, whilst diphtheria is common in India, scarlet fever has never been known to be communicated in that country. Colonies and all new settlements seem to be peculiarly liable to diphtheria. It has appeared in New South Wales and other Australian colonies and in South America, attacking especially the otherwise healthy town of Buenos Ayres. At Lima, however, it has been noticed that the black races withstand its contagion.

It is interesting in this regard to notice that it apparently selects high and well-drained districts as its chief points of attack. In this neighbourhood, Alderley Edge and Bowdon, Cheetham Hill and Kersal, have several times been subjected to its ravages whilst the lower-lying lands in their vicinity have entirely escaped. Dr. Blake, in the "Transactions of the Medical Society of California," mentions that, contrary to the received opinion that diphtheria is peculiar to low, damp situations, the most fatal epidemic in his experience occurred at an elevation of 4,000 feet, in a village surrounded by high hills. Buchanan also, in the report of the "Medical Officer to the Privy Council for 1866", mentions that the disease only appeared at Chelmsford and Dover after general drainage works for these places had been completed. Scarlet fever has also been found to haunt certain districts of England in preference to others; but these Scarlatina-fields, as they have been called, are not at high levels, nor are they those in which diphtheria is especially prevalent.

It seems highly probable that in a very large proportion of cases of diphtheria the source of the poison is some defect in the house drainage; this, at least, is the conclusion to which my own observations have led, and in some instances the disease has been traced, not to the imperfect connection of the house with the general sewage system, but to leakage from private cess-pools. This fact would almost justify a belief in the possible spontaneous origin of the complaint, quite apart from direct communication with other cases of the same disease. It is, however, noteworthy that scarlet

fever also has been observed to spread more rapidly after a sudden fall of the barometer, as though there might be in this disease also some favouring influence due to the impure air of the sewers. Season has certainly an important influence upon scarlatina, the culminating point of its greatest prevalence being invariably in the autumn, and this fact has by some been ascribed to the low barometric pressure usual at this time of the year. There are, however, important reasons for believing that the favouring influence in this case is the high degree of humidity of the air. But diphtheria is not especially a disease of the autumn; it may arise at any season, and in my own experience the most virulent epidemics have occurred in the summer months. Another point of difference between the two diseases is found in the fact that whilst it is not often that scarlet fever is associated with other complaints, diphtheritic affections are commonly met with in the course of various pyrexial disorders.

It is recorded in the report of the Diphtheria Sub-committee of the Epidemiological Society that in the epidemic of 1861 this disease occurred fifty-seven times alone, thirty-four times in association with scarlatina, nine times with small-pox, seven with measles, six with fever, and three times each with ordinary sore throat, croup and catarrh. But there are points of unlikeness in the nature and symptoms of the diseases themselves that are too great to allow us to rank them as varieties of the same species. Thus the scarlet-rash of diphtheria is often absent, and is very variable in the time of its onset; it seldom appears at the outset of the malady, and I have seen it as late as the third week of its course. The period at which albuminuria sets in is also different in scarlatina and diphtheria, often appearing in severe cases of the latter disease within the first two or three days, whilst in the former it seldom sets in until desquamation of the kidneys is commencing.

There is, indeed, an entire absence of definiteness about the duration of the symptoms of Diphtheria, which of itself mark it off from the regular sequence of events in scarlet fever. I have known it last for six weeks and even two months without any evident affection of the cervical glands, but with constant formation of exudation on the fauces or other mucous surfaces. The mode in which diphtheria localizes itself on the mucous membranes, or on the broken surfaces of the skin, would probably be sufficient to separate it from any other disease. I do not know that scarlet fever ever attacks the larynx, but I have seen the diphtheritic

membrane appear in different cases, upon wounds of the skin, upon blistered surfaces, and upon nearly all the mucous membranes of the body, the lips, cheeks, nose, ears, pharynx, larynx, trachea, and bronchi, and on the anus and vulva, and different epidemics have differed strangely in this respect; in one, the throat would always be the point selected for attack, and in another the other mucous membranes. It is somewhat remarkable in the latter case that although the disease was mild, and there were no deaths, the subsequent complications were much more frequent and troublesome. The last point of difference that I shall mention is the fact that while scarlet fever seldom re-appears again in the same individual, diphtheria seems by its first attack to confer no immunity from subsequent seizures. On the contrary, the susceptibility of the throat seems to be rather increased than diminished. Upon taking a survey of all these relations existing between scarlet fever and diphtheria, it is evident that they are distinct diseases, and yet that there is some very close and definite connection between them—wherein the bond consists it is not so easy to point out.

From the manner in which the diphtheritic poison seeks out the highest points of the best-drained localities, it has appeared to me most probable that the virus of this disease must be of a more rarified and subtle nature than the germs that produce scarlet fever. It is possible that the relations of some other ferments may throw some light upon this subject. I would note especially the singular observations of M. Berthelot ("Comptes Rendus;" vol. l, p. 980,) on the glycosic ferment derived from yeast. These observations are important as showing that, from the alcoholic living ferment yeast, a catalytic body may be obtained, which, though it is unable to change sugar into alcohol, yet it will almost instantaneously produce a true fermentative action upon cane sugar.

The following account of these researches may, perhaps, be interesting; after proving that the transformation of cane sugar into moist sugar or glucose is due to the ferment, and not to succinic acid, Monsieur Berthelot thus describes the process he adopted. The soluble parts of the yeast are extracted by maceration in cold water, and subsequent filtration, the filtered fluid containing 1·5 per cent. of soluble matter. When this fluid was placed in contact with an equal part of a solution of one part of sugar in five of water, containing one fiftieth part of bicarbonate of soda, it changed

the sugar at once into glucose without making the liquid acid, and without producing alcoholic fermentation, and without giving rise to the development of living beings. The ferment which produced this change could be precipitated in the form of white flocks, which, when washed with alcohol and dried at ordinary temperatures, formed a yellowish horny mass, whose weight was 1-5th of the total solid contents of the solution. It is a peculiar nitrogenous compound, coagulable by nitric acid and heat, and apparently similar to diastase and pancreatine. After being thus isolated it can be re-dissolved in water and again precipitated by alcohol. These reiterated treatments only weaken somewhat its specific action. It was ascertained conclusively that this substance was secreted by the yeast plant, but that when once produced, it does not require the presence of the living being in order to perform its work.

A similar, though converse action of double fermentation, is mentioned by Dr. Schunck, in his account of "Rubian and its peculiar ferment—Erythrozym." This ferment is a catalytic albuminous substance, similar in its chemical reactions to the glucosic ferment just mentioned, but "with an effect upon rubian altogether sui generis". If a mixture of this body with water be left to stand in a warm place, signs of putrefaction show themselves, and in this state it acquires the power of decomposing sugar into alcohol and carbonic acid. In this case no doubt the germs of the alcoholic torula have been implanted upon the other ferment; but it is interesting to observe that the two processes display this relation.

I do not wish to lay upon these observations a heavier burden than they will justly bear, nor do I venture even to affirm that there is a strict analogy between these fermentative operations and the zymosis of scarlatina and diphtheria, but I believe that it is only by the study of the action of ferments out of the body, and by cautious comparisons between them and morbid zymotic processes, that we can ever obtain a true insight into the pathology of these disorders. It is highly probable that as there exist in nature ferments which are only effective when living germs are present, and others which can act by their own inherent power, so there are disease ferments, which are organized and living, and others which act independently, which have no definite term of life, which are not reproduced, and which in some cases are of spontaneous origin. By far the larger proportion of the zymotic diseases show signs of being pro-

duced by organized living ferments. The peculiar poisons which occasion their symptoms evidently go through certain changes in successive definite periods of time; they grow by the assimilation of food which they find within their hosts; and at a certain period of their growth they possess the power of giving off reproductive contagia. These contagia require a definite period of time in which to incubate, and when developed they produce a disease in every respect the counterpart of the original disorder. Few men accustomed to observe the ways of living beings will hesitate to admit that these disease ferments possess all the characteristics of living germs.

But diphtheria displays very few of these peculiarities; it goes through no definite consecutive changes; it has no regular term of life, and it is with difficulty reproduced by contagion, probably only by direct contact with the person affected, and it is more frequently generated anew from the putrefactive or fermentative operations going on in the decomposing animal contents of cesspools and sewers. If we suppose, by way of a provisional hypothesis, that the processes of the diseases with which diphtheria is known to be associated have the power of producing an unorganized catalytic agent—the diphtheritic ferment—I venture to think that the prominent differences between it and other zymotic diseases are to some extent accounted for, and that the reason of its close relationship to scarlet fever might then be regarded as approximately made out. But we may take another view of the question. Notwithstanding the facts that have been urged to show the divergence of the diphtheritic ferment from the ordinary type of living disease germs, it is by no means impossible that it may yet prove to possess a truly organized character. In this case, the other illustration of a changed fermentative action which has been adduced may throw some light upon the peculiar relations between diphtheria and the other complaints with which it is so often associated. It is probable that in most cases of fermentation the living being, connected with each kind of this action, is of a specific nature, and peculiar to the medium or to the food upon which it feeds. It is hardly needful to instance the acetic, alcoholic, butyric or lactic ferments as samples, according to Pasteur at least, of specific leavens.

But our second illustration points to the fact that different substances will furnish the peculiar food upon which one specific ferment can live and thrive. And it is certain that

in some instances, a ferment either during, or at the end of its operation, will produce the conditions favourable to the growth of another kind of germ. Witness the crowd of vibrios that make their appearance at the end of a mycodermic fermentation, or the production of acetous fermentation after the yeast plant has done its work.

It seems possible that there may be some analogy to this fact in the relations of the two diseases which we are considering. It is not unlikely that any form of the pyrexial state may produce the food most congenial to the diphtheritic ferment, but that the processes of scarlet fever are peculiarly favourable to its production. It is, moreover, worthy of remark, that this explanation would hold good, whether the diphtheritic poison should prove to be an organised germ or an unorganized catalytic agent. In either case the true disease-ferment would find its way from without, and would produce the peculiar symptoms of the disease when it found the substance upon which it could work.

These suggestions are only offered as provisional hypotheses; and, as some excuse for bringing them forward, I will quote in conclusion the words of the great English philosopher, Robert Boyle:—"He that thoroughly understands the nature of ferments and fermentations, shall probably be much better able than he that ignores them to give a fair account of several diseases (as well fevers as others) which will, perhaps, be never thoroughly understood without an insight into the doctrine of fermentation."—Works, ed. 1744, vol. i, p. 476.

FURTHER REMARKS ON THE PERIOD OF INFECTION IN EPIDEMIC DISEASE.

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[*Read February 10th*, 1875.]

The first object in collecting these observations was to define with all possible accuracy the interval between the reception of infection and the appearance of symptoms in some of the more common epidemic diseases. This interval, the incubation period, instead of being uncertain, unimportant, and almost without symptoms, has been found to have definite symptoms, a great influence on the spread of measles, hooping-cough, and diphtheria, and a duration sufficiently constant to be of generic value in the classification of disease.

A remark of Sydenham's, "that no one as yet hath sufficiently studied the various and accurate methods nature useth in generating diseases", continues in force to our own day. The study of all that pertains to the development of disease from infecting particles can only be said to commence with the last century. Dr. Richard Mead, in his "Essays on Poisons", published in 1702, calls attention to the long period of latency in rabies. In "A Discourse on the Plague" (London, 1720), he shows that sudden seizures should be attributed not to fright but to infection, and says "contagion is a real poison"; the whole argument as to contagion is as fully stated as in any recent discussions on cholera or typhus. He is among the first to remark that the practice of inoculation "shows that the small-pox does not break forth before the eighth or ninth day from receiving infection." Till this was known no progress in tracing the ingress of disease was possible. Small-pox and plague were then prominent epidemics; to attain such precise knowledge of the natural

history of one of them was a great gain; without the facts of inoculation, the existence or even the possibility of infection was an inference, and not a demonstration. This inference, by no means generally admitted for small-pox, was of sufficient force to be allowed for diseases quickly communicable. Terror at the swift stroke of plague had impressed people with the idea that all illness was sudden, so that hydrophobia, small-pox, measles, as well as all ordinary ailments, were commonly thought to be owing to the last thing that seemed strange because it did or did not happen. Accurate definition of diseases is a necessary step to the knowledge of their laws of growth. How gradually and slowly the distinctive characters of the most marked and most common of them are separated and recognized is part of our own experience. Enteric fever, so clearly separated from typhus, was formerly confounded with ague, while typhus has quite recently in India (*vide* p. 451, foot note) been detected in what was thought to be remittent fever. The distinction of typhus from plague has been satisfactorily established by one of our members, Dr. Tholozan, and confirmed by a further experience still progressing. Distinct as plague is from typhus, it formerly included that as well as other fevers under its ban. Diphtheria and scarlet fever have passed under its name, and, with scarlet fever, measles; for these two diseases were not usually separated until near the end of the last century; the technical term for measles, "*morbilli*", signifying minor plague, is borrowed from the Italians, among whom *il morbo* signified The Plague.

The shortness of the incubation period in scarlet fever affords a ready means, still serviceable, of distinguishing that disease from measles. These, carefully separated in descriptions since the time of Sydenham, are sometimes confused in practice now as they were then. Richard Morton,* who considered scarlet fever to differ only from measles in the appearance of the rash, and to be wholly the some disease, gives ten cases under the former heading, nine being measles, while the name of confluent measles is emphatically applied to an eleventh history really relating to scarlet fever. At that time every severe and fatal case was referred to measles, scarlet fever being specially constituted, by definition only, a mild disease hardly deserving of classical nomenclature.

* ΠΥΡΕΤΟΛΟΓΙΑΣ, Pars altera: sive, exercitatio de Febribus Inflammatoriis Universalibus, authore Richardo Morton, London, 1694, p. 65.

The epidemic increase of scarlet fever in the eighteenth century, and its association with malignant sore throat, attracted the attention of such men as Huxham, Fothergill, Cullen, and Heberden. The ill effects following upon a treatment in this fever, that had been harmless in measles, pointed to an absolute difference in their nature. Measles had always been so closely associated with small-pox that it was assumed to require a similar period for development, and it soon became evident that scarlet fever, like plague, could be quickly communicated. Dr. Withering determined that the rash of scarlet fever would appear on the third or fourth day after contagion; Dr. Blackburne says it may occur from four to six days after; Heberden mentions the fifth day; and Dr. Willan considers the sixth day as the latest for its occurrence. The longer incubation period of measles, already recognized by Willan, was accurately given by Bateman in his "Practical synopsis," published in 1813.

The practical bearing of this part of pathology was first appreciated by Dr. Maton, at the commencement of the present century; to him is due the credit of recognising as a separate disease, on account of its length of incubation, the rubeoloid exanthem only now receiving a definite position. Dr. Mason Good, who considered the quality of contagion in disease as "a doubtful and perhaps an inappropriate ordinal character", far from admitting this ground of distinction, classed Dr. Maton's cases with scarlet fever as *rosalia*, instead of referring them to the *rubeola sine catarrho* of Willan or the spurious measles of Richard Morton.

The starting point for the present investigation is from an "Essay on the periods of Incubation of the Various Morbific Germs," addressed to the Central Board of Health, by George Gregory, M.D., Physician to the Small-Pox Hospital, published during the cholera epidemic of 1832. Besides the high importance which this subject obviously possessed in reference to the sanitary and police regulation of that time, one principal object of the author "was to direct the attention of observing physicians to this neglected branch of pathology." His observations on ague, as well as on small-pox, are of the highest value; I acknowledge my obligation, and wish to place my contributions on the excellent basis afforded by this essay.

A previous communication to the society (p. 406 of the present volume) chiefly deals with the earlier stages of infectious diseases; it is to the later limit of infection, rather

than to the incubation period, I would now direct attention. The new facts to be brought forward, though not very numerous, are important; they support and define many of the positions already assumed, illustrate the duration of infection in scarlet fever, and supply details of the incubation of diphtheria.

Dr. Gregory's experience at the Small-Pox Hospital afforded abundant evidence that the period of incubation for small-pox was twelve days; or fourteen, "counting," he says, "as I always recommend should be done, from exposure to the *appearance of the eruption.*" He moreover establishes the possibility of variation in this period by cases in which the longest interval following upon a single exposure was fifteen days; the shortest under the same condition was twelve days. These cases show for small-pox what appears from my cases to be true for measles, that a single definite exposure is often followed by an irregular incubation period, instead of serving to fix the normal period.

The scope of Dr. Gregory's subject precludes remarks on the duration of personal infection in convalescents. An example of it, however, is seen in his first case of small-pox. Mary Argent was discharged convalescent from the Small-Pox Hospital, July 6, 1830. She slept that night with her sister, Susan Argent, who sickened for the small-pox, July 19; the eruption showed itself July 21. Further experience at the hospital has obviated this danger. Mr. Marson informs me that some time ago convalescents from the Small-Pox Hospital were traced to their homes, and inquiries made for two or three weeks after their return, without finding a single instance of the disease having been thus occasioned. Have typhus convalescents been similarly watched, to see if on return to their homes and occupations they had or had not given rise to new cases? I ventured to assert, from a single instance coming under my own observation, that the infection of typhus did not cease so soon after convalescence as had been inferred from the limited sphere of observation in hospitals, where typhus patients make no long stay after their rapid recovery. We have not much typhus now in London to test the question, but in the late epidemic at Carlisle there were examples of this mode of conveyance, and one very clear instance of typhus being conveyed by means of *fomites.* None of these came from the Fever Hospital.

It is not contended that personal infection lingers as long in typhus as in scarlet fever; these two diseases are placed

in different categories in this respect; but even if the duration of infection in typhus be comparatively short, its limit is not the less in need of definition.

The incubation period of measles (*morbilli*), rubeola, varicella, and mumps, as previously demonstrated, follows closely that for small-pox, ten days being the shortest, and fifteen to seventeen the longest time, excepting mumps, where it has extended to three weeks. A further illustration of this for measles is given me by Dr. Evans, from St. Thomas' Hospital; a child from a house infected with measles was admitted to the Victoria ward for a burn; ten days afterwards this child has measles and is removed; ten days after this, five children in the ward are ill with measles; in a second outbreak three other of the children sicken on the tenth and twelfth days. Rubeola, so prevalent last summer, has an incubation period closely corresponding to, but somewhat in excess of, that for measles. This, in my first published case,* is proved by all subsequent experience to be not less than twelve days, viz., from November 16th to 28th, a correction required at p. 429 of the present volume; the interval has in all cases exceeded ten days. Some of the later cases show an interval of seventeen, and, possibly, of twenty-one days. The long time of incubation distinguishes this rose rash broadly and absolutely from scarlet fever; it is also a cause of the source of the exanthem being so often overlooked and the eruption attributed to cold, heat, fatigue, or food, instead of to the infection on which this form of rubeola, known as *Roseola æstiva*, is always dependent.

For diseases of a long incubation, as typhus, typhoid, small-pox, chicken-pox, measles, and mumps, infection would seem not to continue long after the special morbid process of each disease is completed. Three weeks from the height of the eruption in small-pox and measles suffices for the cessation of personal infection, allowance being made for delay from complications; care is also requisite that no effete remnant of a morbid action already over, clinging to the skin or to articles of clothing, should convey a disease from which the convalescent is free. Exception may be made to the short period assigned for the termination of infection in these diseases, but there is proof of this being near the usual limit. For small-pox we have the fact that within that time the crusts have separated, and evidence

* "British Medical Journal", January 1870, vol. i, p. 99.

that after this is completed the risk of infection is over.

For measles, besides the general truth that infection is more frequently communicated by those sickening for it than by convalescents, and the instances already given, where, after three weeks, the latter have seemed to be harmless, I have the following crucial instance:—A boy, aged nine years, with eight brothers and sisters, four older and four younger than himself, has measles, on December 7, 1873, while at school; he remains away till December 30, when, exactly three weeks from the height of the rash, he joins all the family at home in the country, mixing freely with the other children by day. He has a separate bed-room; he was washed with carbolic soap on his return, and his clothes efficiently exposed to sulphur fumes; no one took measles, though the disease was epidemic at the time and prevalent in the immediate neighbourhood. All these eight children had measles early in 1875. The analogy of chicken-pox and mumps to small-pox and measles makes it probable that for those diseases also three weeks would quit convalescents from the fear of carrying infection, unless they are sent direct from the sick room to the susceptible.

Very different is the teaching conveyed by the results obtained in the investigation of the group of diseases characterized by a short period of incubation. The old *quarantaine, i.e.*, forty days, or six weeks of isolation for those who had been infected with plague, represents very nearly the time necessary for the strictest caution to be observed after whooping cough, diphtheria, and scarlet fever, in guarding against the spread of these diseases. The six weeks should be reckoned always from the height of the disease, and in scarlet fever, where the earliest symptoms are the most obvious, a longer interval must be allowed.

In whooping cough, precautions should always be taken for six weeks from the commencement of the whoop, and though in some complications the time may have to be extended, it seemed sufficient in the following case for all infection to have ceased:—A boy at school has whooping cough from July 16, the whoop well marked on the 21st; febrile disturbance continued to the second week in August, when the expectoration was free and the appetite returned; he now had change of air, and improved rapidly. On September 5, the family reassembled at home without any bad results. On October 20, this boy has a bad cold, with spasmodic cough, and much viscid secretion; a younger brother, now with him at school, has a cold and cough on

November 4, but neither he nor others with him have whooping cough. Infection may persist for two months.

This relapse of symptoms in whooping cough may occur, as is well known, for months, and certainly after all infection is over. In diphtheria, it is possible that some of the later relapses may be without infection. A patient seen with diphtheria on December 13, probably commencing on the 10th, goes out of doors for the first time on January 24, and again has sore throat, followed by exudation, which disappeared on the fourth day. There is no proof in this case that infection continued throughout; isolation was again adopted during the four days of relapse. We have lately had cases brought before us by our member, Dr. Ransome, where exudation constantly reappeared up to six and eight weeks, and in prolonged disease infection is most likely prolonged also. Cases given in "The System of Medicine" abundantly prove the persistence of personal infection for from four to six weeks; the little girl who took diphtheria on joining her convalescent sisters at the seaside had been separated six weeks. In two of Sir William Jenner's cases infection seems to have been particularly active in the fourth and fifth weeks.

Possibly in some of these diseases, most probably in scarlet fever, infection is given off more at a certain advanced stage of convalescence than in some earlier part of it; when all the functions are more nearly restored to their healthy standard, they may expel the last products of the disease with more activity than when they were in a languid or debilitated condition. For scarlet fever, it has been shown by the two first cases in the previous paper (p. 442) how convalescents may communicate infection nine weeks after the commencement of the disease; in the second case it seemed as if the infection was more active at the end of this long period than at an earlier part of it. For another instance of this I am indebted to my friend Mr. Tweed, of Upper Brook Street:—A lad, aged 14 years, has scarlet fever in the summer in a mild form at school, where he goes through the illness with others. After a month he comes home to his family in London, but at first is kept as much apart as possible. A fortnight after his return, two sisters, aged 17 and 19 years, spend a week with him and then go on a visit to the country. They remain away ten days, and again return home to their brother; on the day week of their return both have scarlet fever; the symptoms were mild, but well marked throughout, so as to leave no doubt

as to their nature. No other source of infection is discoverable beyond this return home to their brother, sixty days after the commencement of his illness. This period was exceeded in the other cases given, but here we remark the infection was inoperative during a previous fortnight. May it be that some infection given off by the brother accumulated in the house, and that the sisters returned to it from the country in a higher state of susceptibility? One of our members, Dr. Elliot, of Carlisle, attests the accuracy of the following facts published by the gentleman in whose family the cases occurred.*

Four years ago a boy from school, convalescent from mild scarlet fever, is sent away a fortnight after his recovery, with one of his brothers as a companion; in six days this one is ill with the disease. Six weeks and a day after this, all desquamation having ceased in the younger one, and both being dressed throughout in new clothes, they return home; again in six days from their return two younger children simultaneously begin with the disease. In watching these two patients most carefully, desquamation was traceable in the mildest of the two cases up to the eighth week; at the end of the ninth week they joined the rest of the family, and no further evil consequences ensued.

More importance is generally attributed to desquamation in the spread of scarlet fever than is its due. As a visible sign concurrent with others less obvious, it is useful in showing that the reparative processes are incomplete, and in directing attention to the skin, to warmth and cleanliness, but a perfectly innocuous cutaneous epithelium may be separating, while from another surface an actively infecting product is being set free. Perhaps the evil resulting from this erroneous view more than compensates for the good, for by directing attention exclusively to the "peeling", which, often active in the second week, may be nearly over in the third, persons are deluded into the idea that infection is then over also, and the disease be again allowed to spread.

The numerous instances of infection in the latter stages of scarlet fever, and the success which usually follows early separation of the healthy from the sick in saving them from the disease, have originated an idea that scarlet fever might

* "On the Necessity of Perfect and Prolonged Isolation for Stamping Out Contagious and Infectious Diseases." Steel Brothers, Carlisle. 1874.

possibly not give off infectious particles or germs until arriving at a certain maturity; consequently, that the first stages of the disease might be harmless. A striking example of the influence of this belief occurred last November at the Epsom Petty Sessions, where a person escaped penalty for removing a scarlet fever patient in a hired fly, by obtaining direct medical testimony that the disease was not contagious in its early stages. That this is an error is clearly shown by cases 7 and 8 (p. 443) in the former paper, where of two children one only is exposed to scarlet fever on February 17th, has rash on February 20th, and communicates it so rapidly to the other that the symptoms of it appear the very next day. Another instance of scarlet fever being communicable on the first day of illness is the following. In a family of nine children, the eldest comes home from a school in which was scarlet fever. Three days after, the mother, in delicate health, is suddenly seized in the night with illness, and the next morning has the rash of scarlet fever. The eldest of the family has already gone away, and the two youngest children are now sent off with the nurse; three elder girls, who had been into their mother's room on the morning of her illness, go to the house of an aunt near; three of the children remain in the house. On the third day two of these are taken ill; on the fifth day, five days after the separation, one of the girls is sent home with scarlet fever; two days after, a servant and the only other child in the house are seized. The two youngest die. The five children who had been separated join the convalescents at the sea side, six weeks and three days after the last seizure, without injury.

The two first of the following cases occurred ten years earlier in houses well situated, but with old brick drains remaining in the basements. The eldest girl of a family of five young children, sleeping apart from them in her mother's room is seized in the night with an illness which next morning is thought to be scarlet fever; that day none of the other children come into the room; lodgings are taken for them in a house opposite the same day. They are sent there with their clothes and all that they want, and no communication is said to take place between the houses; the day fortnight after the removal of the four children, the eldest of them, a girl, is sent home with fever and commencing rash; the three younger, boys, continue well. A month from this time, six weeks from the first illness, the second case being mild and both considered convalescent,

all the family meet at Brighton. Again, at the end of a fortnight the disease re-appears; the second boy, aged two years becomes ill; three weeks after the youngest, nine months old, takes it; lastly, the eldest boy, who had not been kept apart during the stay at Brighton, sickens when his brothers have recovered, the scarlet fever running a mild course. These attacks occurred from July to October, keeping up an anxiety during three months which another three weeks of isolation in the first instance would have obviated.

In the first of these groups of cases we see that personal infection had ceased in six weeks from the illness; we cannot always be sure it is over so soon. We see in the second group that to allow a less period is to let the disease be again spread in a way that is most frequent. In the first group, all who remain in the house take the disease; in both, one of the children removed has it, but on again separating the affected child, all the others escape: the long interval in the second is exceptional, yet any conveyance of infection from the house opposite was not traced. In the first case the disease appeared within six days after separation, an interval corresponding with the usual limit. In the only instance under my own observation, where the symptoms have appeared as late as the eighth day after separation, there had been communication with the infected household during the interval. It is a point of the greatest practical importance that, on removing the healthy from a source of scarlet fever, one week will generally suffice to determine their immunity. Yet I have known children kept from school three weeks, because they had visited at a house where scarlet fever broke out after they had left.

For diphtheria, to determine whether infection has been escaped, a somewhat longer separation is prudent than is required for scarlet fever. An eight days' interval for incubation is proved in Mr. Adams' case, where prompt removal was effected. Details of a similar case will be given, wherein it is not perfectly clear that the interval was only eight days. Abundant evidence of the shortness of the incubation period for diphtheria exists. We know from Sir William Jenner's case in the Children's Hospital, where the disease was found to be in an advanced stage within thirty hours, that the hours of the latent stage may be numbered by units. The two cases published by the author, in the "System of Medicine", article "Diphtheria", show that the shortest period of incubation could not have been less than three days in the first case, nor the longest period more than four

days in the second. But single instances do not seem to fix either the longest or shortest possible periods for diseases so variable in intensity and duration as are scarlatina and diphtheria. The following is an example of the long interval:—In the family of a doctor of medicine, living near the southern boundary of the Fens, a housemaid is ill with sore throat and slightly marked febrile symptoms on July 17, 1874; on July 23 the elder of his two children goes to visit a relative at a perfectly healthy house, well situated on the chalk, in Hertfordshire. On July 30, this child, who had been very lively and well till bed time, becomes suddenly febrile, with vomiting, high pyrexia, and next day enlarged cervical glands; exudation soon appeared on the tonsil and fauces, and persisted for ten days. The child at home was taken ill in the same manner on the same day. Subsequently two other residents in that house were ill, the one with severe, the other with slight sore throat. The possibility of a more than eight days' interval is suggested by these cases; but until some clear evidence of a longer interval is forthcoming we must leave this as a limit, or allow ten days at most for it.

One reason why the duration of the incubation period for diphtheria has been unduly prolonged is because the special exudation cannot sometimes be depended on as characteristic until the third day of the disease; it often does not excite attention till a day or two later; consequently, when the source of infection is known, the interval from exposure is reckoned up to this advanced stage of the illness. A second reason is, that very often the whole time from the commencement of a continuous exposure is reckoned, though the infection may only have been received in the latter part of it. This consideration affects the cases contributed by Dr. Rumsey to the second report of the Medical Officer to H.M. Privy Council. In these cases, one sister takes the complaint exactly a fortnight after the other was seized with it. But they had been together during the illness, the second taking part in the nursing of the first. The first sister also was seized exactly a fortnight after the return home of a brother convalescent from diphtheria, but infection might have been imparted on any day after the first of his return. In diphtheria, as in scarlet fever, infection may be more active at certain stages of convalescence than at others. This appears in some cases given by Dr. Johnson in "The Lancet," January 2, 1875. A girl, aged 17 years, who had been ill ten days before at school, returns to a

healthy country house where are seven younger children; all keep well for a fortnight, when, the elder girl being freer from all signs of illness than on her first return, two of the children take diphtheria and die; the others, separated as soon as these were seized, all escape; yet they had been already exposed for a fortnight to infection from the elder sister.

In the same article some very instructive cases, observed by Dr. Carey, of Windsor, are given. In a family of nine children, two, and the mother, are ill with diphtheria; the mother dies: this is in December. Five of the children are then sent away; the two convalescents and two others remain at home. Of these, the youngest child is seized January 15, and dies on the 23rd; the three remaining now leave the house, which is thoroughly cleaned. The family does not return till April 9. On April 16 a gentleman visits at the house, staying that night and the next. He feels ill on the evening of the day he left; next day he has sore throat, and on the third day diphtheritic exudation. Hence this case might by some be reckoned as of four days incubation, instead of one, or less than two at the longest. A more important correction is required in the last of these cases. On April 23 the whole family again leaves the house. Of two children who go together to a healthy farmhouse, one becomes ill on April 25, or within the usual time for the infection of diphtheria showing itself. On May 11 the other child sickens; but instead of supposing the infection in this case to have been received eighteen days before, and taking this as a proof that it came directly from the infected house, it is easier to believe that the illness of the second child was derived from that of the one then going through the disease under the same roof. These cases require noticing at length, as they are used to support the view of local causes being in themselves sufficient to originate diphtheria. The first link in their causation, as is so often the case, eludes us; but the persistence and fatality of the outbreak can be traced to the existence of a foul disused cesspit under the floor of the infected house. It is argued, from the fact of this cesspool being disused, that infected matters from the sick did not directly enter it and so become a source of re-infection; but it must be conceded that if the air of the cesspool could get into the house and infect the inmates, the air of the house bearing infectious particles could get into the cesspool; indeed, with the alternations of moisture, pressure, day and night temperature, house

fires, and other causes, this interchange must be constantly going on.

The relation to local sanitary defects of diphtheria would seem to be somewhere intermediate to that of typhoid and scarlet fever, less constant and definite than the one, somewhat closer than the other; like scarlet fever, often intensified by the association, yet much influenced by individual susceptibility and not independent of infection, as seen in the following cases:—On January 8th, 1871, a woman, aged forty-six years, returns home to a perfectly healthy house at the west end of London, after nursing a child who died of diphtheria at Teddington the previous day. On January 9th, the nurse has stiff neck and fever; next day, exudation on the uvula and right tonsil, albuminuria, temperature 104·5 deg. Her son, aged seven years, is removed to another part of the house; a grown-up daughter remains to attend in the sick room. Four days afterwards, in the night of January 14th, the boy is sick, and on January 15th, has sore throat, enlarged cervical glands, high temperature, but no rash. In the night of the 15th, the daughter has chills and complains of her throat, next day is febrile, and the day after has exudation on the right tonsil. The fever lasts five days, but is not severe, the skin being pale and moist; both she and her mother had previously had scarlet fever. The brother had a prolonged and serious illness, which during the whole time it was impossible to say was not scarlet fever; there was albuminuria from the second week to the sixth, with anasarca, and hæmaturia at the month end. The pyrexia lasted a fortnight; twice in the course of the illness some irregular rash was noticed; he had never had scarlet fever. One such instance of this ætiological relation of scarlet fever to diphtheria gives great support to the idea of their essential identity: some facts from morbid anatomy point to a close relationship between diphtheria and croup; were we, in consequence, disposed to assume that these two diseases are really identical, the difficulty of admitting the identity of croup and scarlet fever would be an insuperable obstacle.

On investigating the source of these attacks at Teddington, it was evident that the child who died had diphtheria, and not scarlet fever; three other young children were in the house who, not having had the disease, did not then take it, nor was it epidemic near. The sanitary defects at first sight were glaring; ten houses built on alluvial gravel, near together, had no drainage; in the back yards were

open privies. The interior of the house was clean and commodious, and this child, the youngest, never went to the out-door privy; in fact no connection could be established between the local conditions and the occurrence of diphtheria, but this: the grandmother, aged ninety-three, on returning from the back yard, on December 8th, the weather being cold, was seized with illness, and had sore throat; afterwards the mouth showed many white patches, and the breath was offensive. All this had cleared off, and beyond debility, the old lady was feeling pretty well again, when her grandchildren came to visit her in her room in the last week of the year. She had the youngest on the bed to kiss; the child was taken ill on Sunday, January 1st, and died on the 7th of January, with great cervical swelling but without laryngeal symptoms. The depressing effect of cold probably was more concerned in predisposing this aged woman to diphtheria than anything from the foul privy, though the exciting cause may have arisen there. No other inmates of these houses suffered.

The close relationship between scarlet fever and diphtheria is a matter of common knowledge; the same precautions necessary against the one are required against the other; they agree in the circumstances that either aid their favourable progress or are hurtful to it. The precaution necessary to prevent the unintentional spread of these diseases by sending persons among the healthy from a source of infection is that, not only should the full week requisite for scarlet fever elapse, but a period somewhat in excess of this; for while, as in scarlet fever, diphtheria will generally show itself, if it be taken, in three or four days, much clearer instances of it happening after a full week's separation are forthcoming than in the case of scarlet fever.

Scarlet fever affords a strong contrast to measles in all its characters; the interval from exposure to sickening is usually short, and the rash is always an early symptom, so that it ought not to be difficult to separate the sick from the healthy with good prospect of success; and, as a matter of experience, early separation is found generally successful. Moreover, there is this in favour of such a course, that if any of those removed have really received the infection, symptoms of it will appear so soon, that no long uncertainty has to be provided against, and a second separation is sure to be timely.

It is well to state more formally this difference between scarlet fever and measles: on separating children from a

short, definite exposure to measles, no distinctive illness is to be expected until after more than a week's interval, and they are not safe to escape for a fortnight: on separating persons exposed to the infection of scarlet fever a few days, it may be three to five, sometimes a few hours will suffice for the disease to appear: after a week of immunity, we may be satisfied that the disease has been escaped. The utter divergence from, and dissimilarity of measles to scarlet fever cannot be too forcibly insisted on; these two diseases are representatives of wholly different classes of disease, measles bearing affinities with small-pox, chicken-pox, mumps; scarlet fever with plague, erysipelas, influenza, diarrhœa, perhaps also with cholera. No vestige of place for doubt remains as to the close relation of rötheln, the rash to which much attention has recently been called, to measles and not to scarlatina; so the relation to dengue, a rash common in Africa and both the Indies, is clearly and closely to scarlet fever. I doubt if it have more than an accidental resemblance to measles. To speak of any new or anomalous rash as a hybrid between measles and scarlet fever is a confusion of terms and ideas, destructive of what we know, and obstructive of further knowledge.

A popular recognition of these differences is of the utmost importance to the safe management of the sick, and especially to the safety of others. For instance, is the finely diffused rash of scarlet fever the early symptom? Then separate, if possible, others of the family: but, supposing the isolated spots or groups of spots of measles appear, doubtless the previous sneezing or coughing will not have been overlooked, and it is too late to separate others, or at least it is wrong to send them among the healthy susceptible. Again, after the rash of measles is fully out, one day suffices for the child to eat and drink and sit up; care against chill and fatigue is wanted, but the illness has suddenly declined with the throwing out of the rash. In scarlet fever the rash is only the commencement of a series of special injuries to which most organs of the body are liable for a period of three weeks, against which rest in bed for all this time is the best prophylactic; after this the special accidents of convalescence are to be feared for another three weeks. Albuminuria and the dropsy which result after scarlet fever are unknown in or after measles. Measles seldom or never recurs; scarlet fever not unfrequently happens more than once.

An important practical deduction from these facts which,

if acted upon, would soon reduce the spread of scarlet fever to a minimum, and relieve our healthy sea-side towns from one of their greatest risks, is—do not seek change of air too soon for scarlet fever convalescents: they are better in their rooms for the first three weeks; they are not only safer in their homes for the next three weeks after that, but they gain strength just as fast, often faster, than if they had been sent off for change of air, to the danger of the conveyances used; to the danger of the lodgings they go to; and to the danger of all with whom they come in contact. This is of special importance in the management of schools. Much may be done by domestic management in keeping convalescents not only from school, but from play, theatres, parties, and fêtes, until reasonable fear of infection is over. School managers have a duty to protect their schools, they should have power, so as to be able to exclude from school those known to be likely to bring infection with them. It need not be difficult to obtain a written statement of the illness a child is absent for; then let it be incumbent on the manager not to receive that child for six weeks at least after an attack of scarlet fever.

For the nearer allies of each of the diseases already treated of, some variation is noticed from the typical characters just given. Chicken-pox follows small-pox so far as for the sickening to occur twelve days after exposure to infection; but, instead of the rash beginning two days afterwards, it begins at once; and some isolated spot may always be detected as soon as the first symptoms of sickness are felt. There is no more trustworthy guide to so important a matter as the distinguishing off-hand whether it be chicken-pox or small-pox that may have appeared in a family or institution, than to ascertain whether the first spots were preceded by illness or not. I have known chicken-pox to be attended with so much fever, and the spots of so suspicious an aspect, that, but for this circumstance, a fear of small-pox would have remained; also, and this is of greater practical moment, I have known small-pox so masked and modified, that but for the two days of back-ache and malaise preceding the eruption, some of the necessary precautions against so insidious a danger might have been omitted.

There is the same parallel noticeable in the relation of measles and rubeola (rötheln); in the latter the eruption occurs without precedent illness, or with some faintness or catarrh of only one day's existence, instead of three or four.

An allied exanthem, marked by a diffused form of rash, not unfrequently met with in the summer, may be termed *epidemic roseola.* It is quite distinct from rubeola or rötheln, though hitherto not clearly separated from it, and some confusion has entered into the descriptions given of either from want of attention to their distinguishing characteristics. Bateman's description (*op. cit.*, p. 96), applies to the latter; the former may be included in one of his first varieties. Both are readily discriminated from the rose-rashes produced by irritating matters taken into the stomach, as with them the illness follows quickly upon the exciting causes, be they food or physic. Roseola differs from rubeola in aspect, for it does not begin in isolated spots, but a finely diffused redness is noticeable first in the face and neck soon after slight faintness or giddiness has been felt. The small lymphatic glands of the neck are palpably enlarged in both ailments, and there is soreness of the throat. Fuller, in his "Exanthematologia", p. 128, speaks of this sort of rose rash, "as a flushing all over the body, like fine crimson, which is void of danger". This rash may be followed by desquamation, which, rare in measles, is still rarer after rubeola.

Roseola agrees with the scarlatina group in having an incubation of near upon a week; another strong affinity is shown to this class of disease by the occasional occurrence of albuminuria, a point contrasting strongly with the measles group in which albuminuria is not known to occur. I have met with one statement, that albumen has been found in rötheln or rubeola; I suspect here the rash was of the diffused type I wish to discriminate, for I have examined for myself very many cases of rubeola without ever finding albuminuria, but in a less numerous series of this kind it has sometimes been present. The interest attaching to this consideration is very great, for seeing the close connection of rötheln with measles, roseola, as here defined, might turn out to be some derivative of scarlet fever; the more prevalent scarlet fever is, the more does this prevail. Long ago the connection of roseola with summer diarrhœa was noticed by Bateman; the interchange of summer diarrhœa and winter cough is very constant, and has frequently been set forth as closely correspondent. These ailments may seem slight and of no great moment to the strong, but they are serious to the weak, and are the great causes of infantile mortality, especially in great towns.

Albuminuria has been met with in the early stages of this

diffused form of exanthem, even where the febrile disturbance has been slight; muscular and articular pains have followed some of the more marked cases.*

The infection of catarrh, and of sore throat under its two distinct forms, the follicular and parenchymatous, with the relation to influenza, alluded to in my former paper at p. 444, may be illustrated by a few cases. On December 11th a little girl comes home from school with bronchial catarrh, and sleeps in the same room with a sister, four years old; it was noticed at night how quick was the breathing of the elder girl, and how quiet that of the other. The next morning, by 7 a.m., the younger child is breathing rapidly, has coryza and fever by evening, on the second day catarrh, on the third congestion of the lung, and a six days' illness. Two other children of this family on December 13th and 14th were similarly attacked. The same winter, in another nursery, an elder child introduces catarrh on December 8th; on December 10th the younger sister, two years old, has a similar cough; on December 13th a boy three and a half years old becomes suddenly febrile and has a bronchial attack lasting six days. In both these nurseries the under

* I may add some recent details: on May 8th, 1875, a little girl of two years old felt tired and was restless at night; on the 9th a fine bright redness was noticed in large patches on the back, chest, and neck; a vivid patch encircled one wrist. The redness became more diffused at night, the temperature was not quite 100 deg. The next day the redness was fading, and the temperature normal; the small cervical glands were palpable, there was no sore throat, but some mucus four days after. Other children in the family had no rash. In another house, on June 16th, a maid-servant, aged fifteen years, in regular health, felt giddy while dressing, and vomited; a fine rash was discernable over chest, neck, and arms; face very red, eyes suffused, pulse 100, temperature 99·2 deg.; 17th, rash fading, tongue clean, slight redness of palate and fauces, slight fulness of tonsil just visible, one small gland felt at angle of jaw. Pulse 80, temperature 99·6 deg., albuminuria. 18th, feels well, albumen much increased, it lasted a week. On the 24th, the lady on whom she attended felt faint, and was sick at noon; suffusion of face by evening; the rash faded next day, but she had aching of the limbs and restless nights. On May 29th, two young married people came from different parts of the country to attend an evening reception at the Foreign Office; on the 31st, the lady has a fine rash on the chest, neck, and arms, with no rise of temperature; next day in bed with more rash, temperature 99·6 deg. The gentleman complained of sore throat, and next day is covered with finely diffused red rash; he kept in doors, but not in bed, and the temperature rose to 101 deg. Afterwards severe pain in the limbs, and loss of sleep. These two cases, though they had not previously had scarlatina, and did not communicate it to young children in the house, very much resemble some cases of modified scarlatina, transmitted by milk or cream, during the same season in London. The others were simply roseola, as my second published case of scarlatina ("Temperature Observations," p. 18) proved to be, for the child afterwards took scarlet fever.

nurses were seized, during the week they were in attendance upon these children, with follicular tonsillitis, coming on with intense aching of the back and limbs, and resulting in the formation of several small ulcers on the tonsils in the one girl, and a larger round ulcer of one tonsil in the other. The febrile period did not exceed three days. This form of catarrh is often epidemic, it has a short incubation period, and is associated with influenza; it begins with elevation of temperature, often subsiding on the third day; there is generally some enlargement of the lymphatic glands in the neck. I once found the mother, while in close attendance on a child dying with laryngeal croup, suffering this form of follicular tonsillitis.

The parenchymatous form of tonsillitis is associated with conditions more limited in their operation, whether individual or local; the two forms are seldom met with together, and rarely pass the one into the other; when transmitted by infection the latter inclines to the longer period of incubation, extending from two to six days. Last spring, in a good country house with but one sanitary defect—that of an indoor water-closet connected with a closed receptacle, which had not been recently emptied—a boy, aged eight years, has fever and sore throat on March 10th; there was dysphagia for a week. No sooner is this boy convalescent than his mother, who nursed him, suffers with sore throat and great prostration on the 25th; her illness lasted a fortnight. On the 2nd of April a youth in weak health comes to the house, he is seized on the 8th with sore throat and fever. The illness lasted ten days, though during his illness the defect in the water-closet was remedied. The patient left his room on the 18th, and went out of doors on the 20th. On the 22nd he visited a family living at an isolated house two miles off; on the 24th a servant there complained of her throat, and is ill on the 25th with quinsy. At the same time, a servant at the first house has quinsy. Two children of different families visiting at the second house have sore throats on the 26th and 27th; another child then with them soon suffers, and each of these three children, two in different parts of the country, and one on returning to London, become centres from which other attacks proceeded. Stomatitis in the young is communicable by infection, and is more closely related to this than to the catarrhal form of inflammation. I have notes of the visit of a healthy girl, five years old, on March 23rd, to a house near London, where the children had this complaint.

The little girl returned home on March 25th; on the 27th she became ill, next day small ulcers appeared in the mouth similar to those in the other children, and the lymphatic glands were enlarged for some days longer.

Relapsing fever has now been produced by inoculation, the incubation being five days. It is only during the presence of *spirillæ* in the blood that it is infective.

Plague, on its re-appearance last year, was studied at Benghazi by Dr. Laval, Surgeon-Major of the French army in Algeria, at the cost of his life. The incubation period was from two to four days; the attacks were sudden, with high fever in the first few days, the temperature soon reaching 102 or 104°, the pulse 100 to 130 or more; the glandular swellings began on the first day.* Dr. Augustus Bozzi Granville, in his "Autobiography" (London, 1874, p. 137), describes how on reaching Constantinople, many years ago, landing at Tophaneh in the forenoon, he awoke with tremor and headache in the course of the very first night, the swelled glands characteristic of plague appeared in the axilla on the morning of the third day, the fever and delirium left on the morning of the seventh day. He was convalescent in a fortnight; he also relates the self-inoculation of Dr. Valli, who had not been in the Plague hospital three days before he contracted the disease.

Yellow fever has recently been most accurately observed in America, especially by Drs. J. C. Faget, and J. Jones, of New Orleans, and by Dr. Sternberg, of the United States army. The temperature-curves of 183 cases show a single paroxysm, highest at the outset of the disease (103 to 105° or more), rarely extending to the third day; the pulse falls before the temperature, and sometimes becomes slow, one attack in the vast majority of cases protects from a second. The incubation period was mostly close upon five days, often considerably less; there are exceptional cases of eight days, but the danger of infection attaches more to places and things than to personal transmission.

Dengue has been proved thermometrically, in the epidemic of 1873, to have a very sudden ingress, a remission on the

* This feature is noted by Surgeon-major W. H. Colvill, in his report to the British Consul, at Bagdad, on Plague in Mesopotamia, in 1875. Glandular tenderness sometimes began with the fever; enlargement of the glands was noticed before the fever in one case, in this the temperature was 100 deg. on the fourth day; some of the temperatures taken evidently denote collapse. A very clear instance of incubation not exceeding forty-eight hours is given by Dr. Colvill, proving the disease not to be marsh fever.

third day, and one exacerbation on the fourth day; the temperature elevation, both of the ingress and the culmination, is from 102 to 103°; the variations of the pulse exactly correspond with those of the temperature; there is swelling of the lymphatic glands. Sudden faintness, vomiting, chill and high temperature precede the rash by a few hours only; some fine desquamation follows; relapses are not infrequent, and infection lingers for two months. Persons who, in my own knowledge, have had measles, are not protected against attacks of dengue. The liability to recurrence in this disease enables me to agree with Dr. Seguin, of New York (to whose courtesy I am indebted for these new facts) in considering dengue a malarial epidemic; he thinks it may take its place "with intermittent, remittent and hay fever, influenza, true pneumonia, etc., a group which will grow larger as our knowledge of ætiology will enlarge."*

Cholera incubation lasts from a few hours to four days; the premonitory diarrhœa, properly a part of the disease, a few days only. The detection of albuminuria by Dr. Schlimmel,† several hours before other symptoms of cholera appear, tends to limit more closely the period of incubation.

I am permitted by one of our members, now present, to record these facts as they befel in his own family:—In 1853 three gentlemen from different parts of Kent met in London, and dined at the Nag's Head, in the Borough, on Friday, October 21st, the landlord's wife being then ill with choleraic diarrhœa. The elder of the three, who passed the night in the hotel, returned home on Saturday, was seized with cholera the same night, or morning of the 23rd; he died on October 27th; next day, the 28th, his wife, who nursed him, was seized, and died on the 31st. A friend, who came to the house on the 27th, staying only a few

* "Medical Thermometry and Human Temperature", by E Seguin, M.D., New York, 1876, p. 143. From this perfect store of information, received while correcting this sheet for the press, I extract—"DENGUE (Syn. *dandy fever, break-bone,* etc.) Apparently a purely eruptive, epidemic disease of the warm climates. By its eruption often mixed with other epidemics due to summer heat; looks like measles or scarlatina by its eruption; by its pains and swellings of the joints, like rheumatism; by its glandular symptoms like typhus; by its mode of invasion, like yellow fever, by its initial chill, like intermittent, and by its periods like remittent. Thermometry takes it out from that hybrid status, and sets it up as a malarial entity which has its own thermonomy, viz.: invasion too sudden to be noted previously to its acme. Two paroxysms separated by a short remission, and lasting from five full days to seven short."

† "Du Présage et de l'avortement de l'imminence Cholérique." Rotterdam, 1874, p. 32.

hours, but visiting the patient, was attacked on the 30th, within sixty hours of entering the house, dying on the same day. An old man-servant, well on the 30th, was seized with cholera on the 31st, and died the next day. Of the two younger men, returning to their homes on the Friday and Saturday, one became sick before he reached home, the other was taken ill at 7 a.m. on the following day, and had a serious illness; both recovered. There was no extension of the disease from any of these houses.

An outbreak of cholera in Normandy has this history:—In 1848 a fishing lugger, l'Étoile of Fécamp, had to put in to Dunkerque, when cholera was there, leaving on November 9th, and arrived on the 10th at Fécamp with three of the crew ill of cholera, one was taken to the hospital, two to friends in Ypport; the youngest, a lad already algide, was placed in his mother's bed beside her; she was seized next day, and died in twenty-four hours; a few days after a young sister was taken ill and died rapidly. The disease then spread to the houses near, and soon to the whole village.

Kennedy, in "The History of Contagious Cholera" (London: 1832), says the first fatal case in England, that of Robert Henry, a Sunderland pilot, began August 1831, the day after he had piloted a foreign vessel that had come from an infected port of the Baltic. The conveyance of cholera from Newcastle to Gateshead, separated only by the Tyne, is traced to a Mrs. Hindmarsh, who visited an infected district of Newcastle on December 14th. "About eleven o'clock of that night she was seized herself with symptoms of cholera, and on Thursday, the day ensuing, she died." On the 18th the husband goes to lodge with his brother-in-law and two sons in a healthy part of the town; he has some diarrhœa at the time, and takes a chest of clothes with him. On the 26th, one of the sons in this house is attacked, and on the same day the father, and also a second son, who left home the day before to commence work at Shields, all died; the first on the evening of the 26th, the father on the 27th, and the second son at Shields on the 28th of December. The mistress of the house at Shields, where this young man died, was taken ill four days afterwards, and died the next day of cholera.

In these, as in many other instances, very sudden effects follow on first exposure to infection of great intensity, while in some subsequent seizures an interval of three or four days is found. Hence an inference, supported by some in-

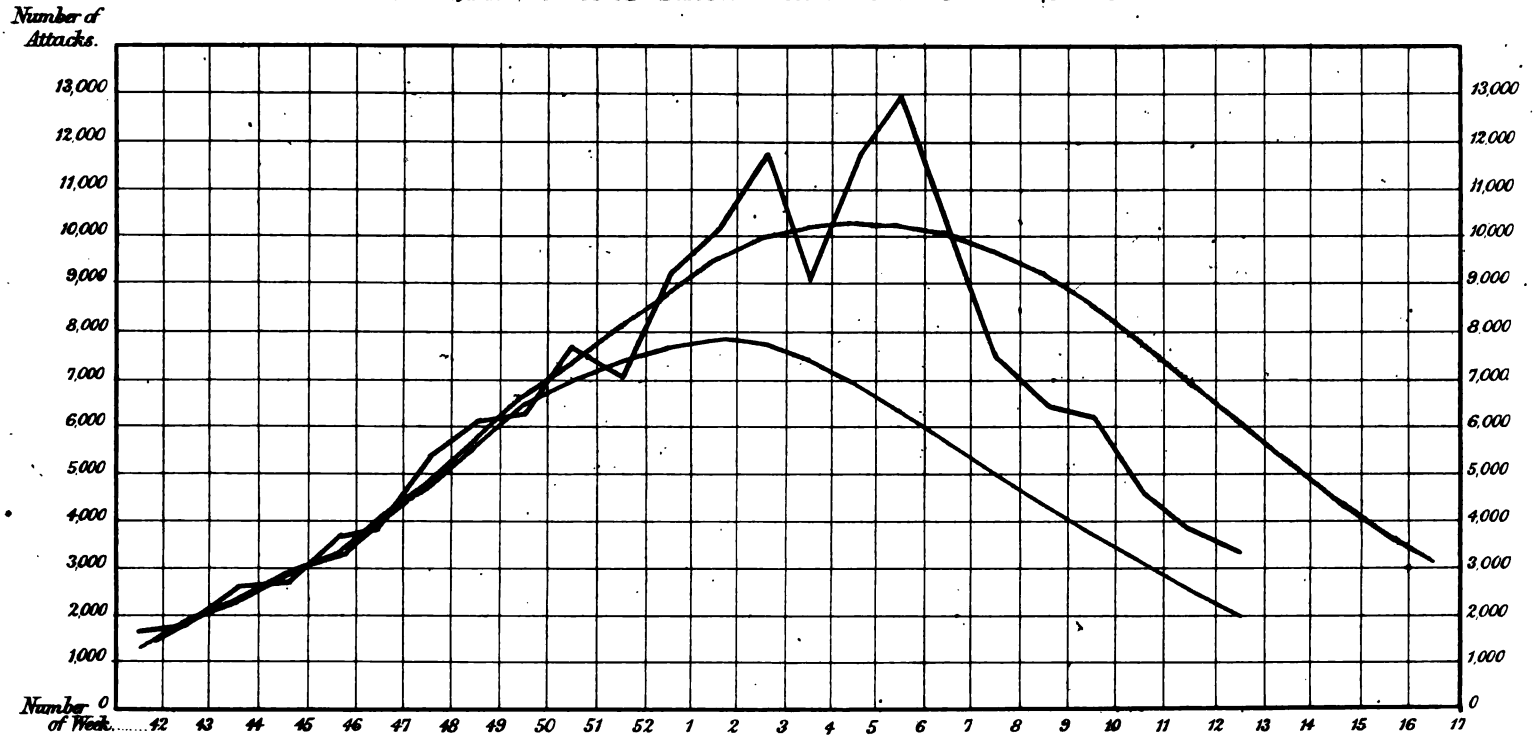
conclusive experiments, that the infecting particles might undergo some change of state, after being thrown off from the body of the sick person and before entering that of the recipient, analagous to what we suppose for the period of latency within the body. The fresh ejecta of cholera patients are not strongly infective, they become so after decomposition; whether this sets free the infecting particles, or whether they are increased by special fermentation is uncertain. Such special augmentation in hot climates would seem a necessary assumption for yellow fever. Any possible increase in the intensity or quantity of infection in this way would probably be restricted within narrow limits that might come within the scope of the present inquiry. There is, however, another possible development of infecting material quite different to anything here treated of; alternating with outbursts of epidemic energy are times and seasons of quiescence for infective germs. We know how for long periods infection may be restrained from action, as that of vaccinia in glass tubes, and we can understand how it may again become active. I would withhold the term of *incubation* from these uncertain periods, and using the term *dormant* for certain conditions under which infection may remain inactive, restrict the use of incubation to the development of disease within the body, and the terms *latency* and *invasion* to the two stages of incubation.

Hydrophobia and ague point to the possibility of infection lying dormant for an uncertain time within the body. Dr. Gregory gives, in his essay, a case of nine month's interval in each of these two diseases. He tabulates thirty-one cases of hydrophobia, and says, "From this table it appears that the average period of incubation is forty-five days; the minimum twenty-one days; the maximum, nine months". Some instances of a longer interval are on record. It is of more importance to direct attention to one or two cases which show the disease may appear in less than three weeks. Dr. Fletcher of Broxbourne, has just informed me of a boy bitten on the morning of April the 1st, who first noticed a difficulty in drinking on April 18th; he saw him with slight choking spasms on the 19th; on the 20th the patient was removed to the Hertford Infirmary, where he died on the same day. The shortest time for incubation of hydrophobia that I have seen published was a fortnight. The more usual period is from four to six weeks.

1.

COURSE OF CATTLE PLAGUE, IN 1865 — 6.

RECORDED ATTACKS BLACK — CALCULATED SERIES RED & BLUE.



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II.

LY, IN 1849.

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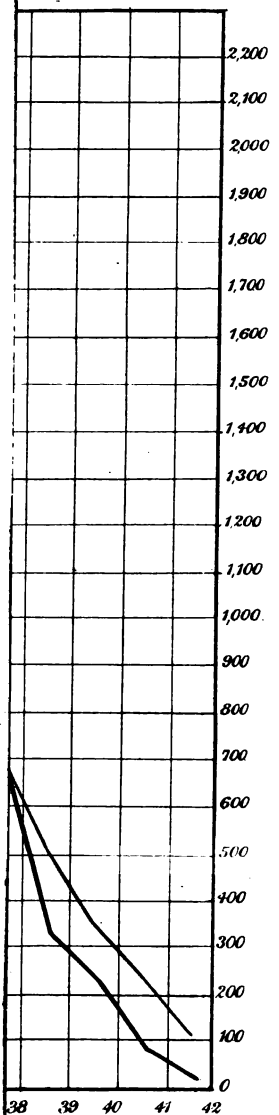

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IN 1854.

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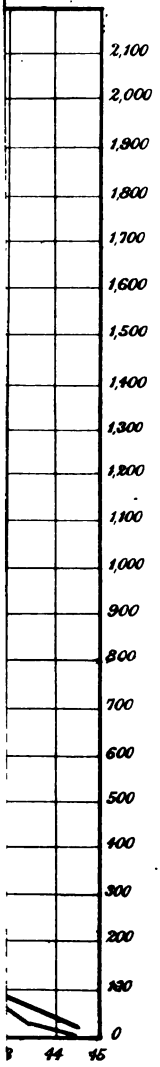

Ford St. Covent Garden.

SOME ARITHMETICAL CONSIDERATIONS ON THE PROGRESS OF EPIDEMICS.

By G. H. EVANS, M.A., M.D. (Cantab.),

Lecturer on Public Health at Middlesex Hospital Medical School.

In 1866, during the height of the ravages of the cattle plague, a statement made by Mr. Lowe in the House to the effect that there was no reason why the terrible law of increase which had prevailed hitherto should not prevail henceforth was controverted by Dr. Farr, in a letter to a daily paper, in which he showed that the real law of this epidemic implied that the ratio of increase went on rapidly decreasing, until the ratio itself was a decreasing one. Applying the law of gradually decreasing increase obtained from the earlier weeks of the epidemic, he calculated its probable course, which, as will be seen from Plate I, very nearly resembled its actual course. Now, it seemed to me, that it would be very interesting to ascertain if by the use of this method we could learn something more about other epidemics, if, in fact, epidemics usually followed this course of gradually decreasing increase, and if we could at any early part of their course, predicate the period at about which they would culminate, and the rate of their subsequent decline. With this view I have endeavoured to investigate a few epidemics of which the progress is known, and to ascertain whether they followed a similar course to that taken by the cattle-plague, and whether this method of calculating the probable course were applicable also to them.

Perhaps it will be as well, in the first place, to describe as briefly as possible the method of calculation which I have received at second hand, through a member of the society, who had obtained it from Dr. Farr. I will take the instance of the cattle plague. Take nine weeks of the early course of the epidemic, in three groups of three weeks each: find the deaths per week in each group: find the number by which

you must multiply the first average to obtain the second, and the number by which you must multiply the second average to obtain the third: or, as a simpler process, take the difference between the logarithms of the first and second averages, and between the logarithms of the second and third averages. The first of these differences we will call δ^1, and the difference between these two differences, which should, to bear out this theory, be a negative quantity, we will call δ^2. We have now the data for constructing our series. The average of the first three weeks is our starting-point, and represents the centre week of those three. The next number in the series is obtained by adding to the logarithm of our first number, a number composed of $\frac{\delta 1}{3}-\frac{\delta 2}{9}$, remembering that δ^2 is a negative quantity; we continue to add to the logarithm for each place in the series a number gradually diminished by the addition in each place of $\frac{\delta 2}{9}$: after a time the number to be added becomes negative, and our series gradually diminishes. It is obvious that this method of calculation will not produce a gradually diminishing series unless δ^2 be a negative quantity, that is, unless the data from which you calculate the probable course of an epidemic be taken from a period at which the epidemic is advancing with a gradually decreasing rate of increase. Now the question is, can we generally find, early in the course of an epidemic, data for calculating its probable course? I will give you now the instances in which I have endeavoured to apply this method. I have selected, as far as possible, such epidemics as are but little affected by sanitary measures, most of my instances being from the weekly returns of deaths in London from scarlet fever.

I will begin, however, with the cattle plague. It will be seen from Plate I, that a calculation based upon data obtained from nine weeks' reported attacks ending December 30, 1865, produced a curve for the epidemic which reached its acme in the fourth and fifth weeks of 1866, and then gradually declined. Now the largest number of reported cases appeared in the sixth week of 1866, and the real decline of the epidemic was more rapid than the calculated decline, a result very reasonably attributed by Dr. Farr to the effect of slaughtering in the latter weeks of the epidemic. On the whole, the epidemic seems to have followed the course indicated by its commencement. I may remark that the calculation could not include the first week of reported attacks, ending October 25, as in that case δ^2 would

have been a positive quantity, and the calculated series would have gone on increasing indefinitely. Supposing, on the other hand, that the calculation had been commenced with the week ending November 11, a very different curve would have been obtained (indicated in blue ink on the Plate), arriving at its summit in the week ending January 20, and after the first nine weeks, very little resembling the real course of the epidemic.

In the case of the cholera deaths in London, in 1849, it will be seen in Plate II that using nine weeks of the epidemic, commencing with the second, a curve is obtained, which neither in height nor in position of highest point, well represents the course of the epidemic. In this case the first week had to be neglected, as otherwise δ^2 would have been a positive quantity.

In the case of the cholera deaths in London in 1854, it will be seen by Plate III that the calculated series very nearly resembles the real course, but then it must be remembered that the nine weeks from which the data were obtained bring us to the acme of the epidemic, which indeed only lasted eighteen weeks altogether.

I now come to the scarlet fever epidemics, where I get my figures from the "Weekly Deaths from Scarlet Fever in London in the Thirty-two years 1840-71." I have taken the years in which scarlet fever deaths rose considerably above the average; and I have endeavoured, with the help of the charts which I now show, to find whether at any early period of these epidemics, their course furnished data for constructing a curve resembling the real course of the epidemic.

First: in 1848 (see Plate IV), although the fever appears to have become epidemic about the twenty-first week, its course was at first so erratic that we have no data for calculation. On using the nine weeks from the 28th to the 36th inclusive, we get a curve indicated by a blue line, which certainly resembles the course of the epidemic. On using the nine weeks from the thirty-second to the fortieth, inclusive, we get the curve represented by a red line, which is far apart from the actual course during the early part of its decline, but comes very close to it at the end. Both these curves pointed to very nearly the same time as the acme of the epidemic, in from the forty-second to the forty-fifth week, the highest actual numbers being in the forty-first and the forty-third weeks.

In 1854 (see Plate V), the early course of the epidemic

was so erratic that any calculation made would lead to a never-ending epidemic till we used the nine weeks from the thirty-fifth to the forty-third, the latter being very near the apex of the curve. Here, the calculated series singularly resembles the actual course of the epidemic in its decline.

In 1858 (see Plate VI), the calculation based upon the nine weeks from the twenty-ninth to the thirty-seventh gave the blue curve, while one based upon the nine weeks commencing with the thirtieth gave the red curve, which, although never attaining the same height as the actual epidemic, culminates half-way between the two highest points of the epidemic, and keeps extremely close to it through most of its decline. Here, with data supplied by the thirty-eighth week, it might have been predicted that the epidemic would probably commence to decline about the forty-fourth or forty-fifth week, and that it would decline at a certain indicated rate.

In 1869 (see Plate VII), calculating on the basis of the first nine weeks in which the death-rate rapidly rose (the twenty-ninth to the thirty-seventh), we get the red curve, which very fairly represents the course of the epidemic; by using a later series of nine weeks (the thirty-second to the fortieth), we get the blue curve, which in some respects more nearly represents the actual course.

Take the case of the epidemic of scarlet fever in the latter half of 1874. The average number of scarlet fever deaths per week in the first twenty-one weeks of the year was unusually low, only 17.7. After this time, it was never under 30, and slowly and steadily increased up to the forty-third week, when it reached 128. Now, taking our epidemic as commencing at the twenty-second week, let us try and apply Dr. Farr's method. If we commence our series of groups with the twenty-second week, we get an indefinitely ascending scale. Commencing with the twenty-third week we get a similar result, the same with the twenty-fourth and twenty-fifth. With the twenty-sixth we get a very short series, which never gives a greater number than 66, and begins to descend at the thirty-sixth week. With the twenty-seventh we are again met by the positive second difference, meaning a continuous ascent, and it is not till we begin with the twenty-eighth week, that is to say, it is only when we use the nine weeks representing the seventh to the fifteenth of our epidemic, that we can calculate anything like an approximation to its course. We then get a

curve which reaches its highest point at the same time as the actual epidemic, and which closely resembles in its descent the descent of the epidemic.

Let us just enquire whether in any other way it might have been possible to predict the culminating point of this scarlet fever epidemic. On looking back at the weekly returns of deaths from scarlet fever in London we find that in those years in which scarlet fever may be said to have been epidemic, the highest death-rate occurred in the forty-first week in 1844, in the forty-first week in 1848, in the forty-fourth week in 1852, in the forty-seventh week in 1854, in the forty-seventh week in 1858, in the fortieth week in 1863, in the forty-second week in 1868, in the forty-eighth week in 1869, in the forty-first week in 1870. In the thirty-two years from 1840–71, the forty-third week had the highest number of deaths recorded, and the forty-first the second highest.

And now, you may fairly ask, what is the result of these calculations. Well, I may say, very little result at all. It is difficult to say how, in the cases where this method produces a curve like that of the epidemic, we are to ascertain exactly at what period of the epidemic we are to take our data. Even when we find an epidemic growing with a gradually diminishing ratio of increase, we are not certain that that ratio will continue. On the whole, I am rather disappointed; for I thought at first that it was likely that some indication might be found pretty constantly of the period in an epidemic when a calculation of its course might be made. There is no doubt that this mode of calculation represents correctly the course during the nine periods of data-giving observation; and therefore that it would hold good for the entire epidemic if it continued to progress on the same conditions as at first. But it appears that it is by no means universally applicable, and that in many of the cases where the method is applicable, the prediction could hardly be made sufficiently early to be of much value.

A BRIEF ACCOUNT OF THE PROCEEDINGS OF THE INTERNATIONAL SANITARY CONFERENCE HELD AT VIENNA IN 1874.

By E. C. SEATON, M.D.,
One of the British Delegates at the Conference.

The proposal for this Conference emanated from the Austro-Hungarian Government, and was made, as explained in the despatch of Count Andrássy in which it was conveyed, with especial reference to epidemic cholera. In this despatch the insufficiency of the means heretofore adopted for preventing the extension of cholera was pointed out, and the necessity for a complete scientific investigation of that disease on a scale not hitherto attempted was dwelt upon. Adverting to the Conference which had been held on the same subject at Constantinople in 1866, its valuable suggestions, and small results, the Austro-Hungarian Government proposed a further Conference of delegates appointed by the Governments of the several States of Europe, and of certain extra-European States specially interested in the matter, to be held at Vienna, for the purpose, in the first place, of revising the conclusions, scientific and practical, of the former Conference, and of establishing, if possible, a complete understanding between the Governments represented as to the national and international measures to be adopted by them for preventing the spread of cholera; and, in the second place, of considering the advantages and means of establishing a Permanent International Sanitary Commission, for the more complete study of cholera and other epidemic diseases, and the means of preventing their propagation. They proposed to reserve to the Conference the faculty of entering or not, as it might see fit, on the subject of quarantine as applied to other diseases than cholera. It was part of the proposal that the execution of whatever measures the Conference might decide upon should be assured by an International Sanitary Convention.

The invitation of the Austro-Hungarian Government was, after hesitation, I believe, on the part of some of the Governments invited, eventually accepted by all, and every independent European State was represented at the Conference which subsequently assembled. Luxembourg, Switzerland, Servia, Roumania, none of which had been represented at the Conference of Constantinople, were represented at that of Vienna; and Norway, which had only been represented at Constantinople in conjunction with Sweden, was now separately represented. Of non-European States, Persia and Egypt were represented, as at Constantinople. The United States of America had been invited on the present occasion to send delegates, and had accepted the invitation, but no representatives appeared.

The number of States represented was twenty-two, Austria and Hungary being counted separately. Each State was allowed to send as many delegates as it thought fit, but could dispose of only one vote. This arrangement was different from that which had existed at the Conference of Constantinople, where each State represented had two votes, and the voting was individual. To the present Conference Austria and Hungary between them sent nine delegates (without including the President, the Baron von Gagern); France and Spain sent each three; Germany, Belgium, Egypt, Great Britain, the Netherlands, Russia, Switzerland, and Turkey each two; the remaining States—Denmark, Greece, Italy, Luxemburg, Norway, Persia, Portugal, Roumania, Servia, and Sweden, each one. The total number of original delegates was thus forty-one, exclusive of the President. The original delegates of Sweden and Norway being each obliged to leave before the Conference was over, were replaced by others; so that forty-three persons in all, exclusive of the President, took part in the deliberations.

The constitution of the Conference of Vienna differed in a very important respect from that of Constantinople. Of the thirty-seven delegates at Constantinople a considerable portion were not of the medical profession, fourteen being diplomatists; and there were three States—Belgium, Denmark, and Sweden with Norway—which had no medical representative at all. At Vienna every State was medically represented; and there was but one original delegate who was a diplomatist, M. de Ring, the Secretary to the French Embassy at Vienna. Subsequently, on Dr. Berlin, who was the representative of Sweden, being obliged to leave, his

place was taken by M. Kleen, the Secretary to the Swedish Embassy at Vienna. Of the remaining forty-one delegates, all were medical men, or holding office in the public sanitary service of their respective countries.

The Conference which assembled in Paris, in July 1857, for the consideration of quarantine in relation to cholera, yellow fever, and plague, occupied in its deliberations eight months. That held at Constantinople in 1866 occupied also nearly eight months. The Conference at Vienna, in 1874, was formally opened on July 1, and closed on Aug. 1 of that year. In the thirty days which intervened there were eighteen sittings of the full Conference, besides numerous meetings of the several committees, to whom had been intrusted the preliminary consideration and preparation for discussion of some of the more important questions on which the Conference had to decide. No Conference could have worked harder, and with more resolute determination to get its work done: nor would this have been possible within the time in which it was accomplished if it had not fallen to be done mainly by men to whom the various questions to be settled had been long familiar.

The Conference, in proceeding to its deliberations, followed a programme which had been prepared for it, and had accompanied Count Andrássy's despatch. The first part of this programme consisted of scientific questions in relation to cholera—eleven in number—the consideration of which involved a review, from our present state of knowledge, of the scientific conclusions concerning that disease arrived at by the Conference of Constantinople. The conclusions of that Conference had been separately printed for our use; and the course agreed upon was that, as each question of our programme was brought forward, the President should read all those parts of the conclusions arrived at at Constantinople which referred to that question, and then take a provisional vote without discussion. If the vote were one of unanimous assent to those conclusions, the Conference passed immediately to the next question; but if the assent were not unanimous, if a single delegation even abstained from voting, the discussion was held to be open. This mode of proceeding worked admirably, and was productive of much saving of time. I will now state to the Society, as succinctly as I can, the conclusions arrived at by the Conference of Vienna on the several preliminary scientific questions submitted to its judgment.

The first three questions had reference to the origin of cholera, its transmissibility by man, and by goods which had been employed for personal or domestic use. The Conference unanimously agreed with that of Constantinople that the origin of epidemic cholera was in India, and that it there exists permanently in an endemic state; that it is probably endemic *only* in India, though our knowledge concerning some of the countries immediately bordering thereon does not admit of this being positively asserted; that it has never been observed in a really endemic state (a state to be carefully distinguished, of course, from an origin from secondary foci) either in Europe, or in Turkey in Asia, or in the Caucasus, or in the North of Africa, or in America; and that though the possibility of its becoming acclimatized in any of these countries is not to be rejected, such event is to be held as extremely problematical. Our Conference accepted, also, with certain qualifications recognising the influence of localities, the conclusion of the Conference of Constantinople that cholera is transmissible by man coming from localities infected with the disease; that the rapidity of its transmission is in correspondence with the activity and rapidity of human communications; that the arrival in a place of a single individual affected with cholera may be the means of developing an epidemic; and that probably even the so-called premonitory diarrhœa of cholera may be the means of transmitting the disease. Further, it held that cholera is transmissible by goods coming from an infected place which have been employed for personal or domestic use, especially such as have been used by cholera patients.

The fourth question put to us was one which had not been discussed at Constantinople—the transmissibility of cholera by articles of food and drink. The transmissibility by drinks—and especially by drinkable water—was affirmed unanimously by us. The answer given, after much discussion, by a majority of eleven delegations against seven, to the other part of the question was that the Conference, having no conclusive facts showing the transmission of cholera by food, did not feel itself authorised to give any formal decision.

On the next question, whether cholera may be imported by living animals, a conclusion was, after discussion, arrived at which differed in form, but scarcely more than in form, from that adopted at Constantinople. It was " that there exists no proof in support of the transmissibility of cholera

from animals to mankind, but it is very natural to admit the possibility of it." This was carried by the vote of ten delegations, two voting negatively, and six abstaining from voting.

The sixth question, that of the transmissibility of cholera by merchandise, was answered, as at Constantinople, to the effect that there was an entire absence of any proof of such transmission, but that the possibility of its occurrence under certain conditions must be admitted. This was carried by the vote of thirteen delegations, five abstaining from voting; it would unquestionably, had it been limited to the first clause, have been adopted unanimously.

With regard to the transmissibility of cholera by corpses of cholera-patients—which was the subject of the seventh question—there was unanimous accord with the decision arrived at by the Constantinople Conference, that, though there was no conclusive evidence of the fact, prudence required that such corpses should be considered as dangerous.

The seventh and eighth questions had reference to the transmissibility of cholera by the atmosphere, and the action exercised by the air on its transmissibility. These points were decided with unanimity in the same sense as at Constantinople. The conclusions were to this effect: that air is the chief vehicle of the generative principle of cholera, but that the transmission of that disease by means of the atmosphere is limited in the immense majority of cases to a distance very close to the source of infection; that none of the facts which have been adduced to prove transport of the cholera infection by means of the atmosphere to a distance of one or more miles, is sufficiently conclusive; and that, in short, there exists up to the present time no proof whatever of the propagation of cholera to any considerable distance by the atmosphere alone; that, further, it was a law without exception that no epidemic of cholera had ever been propagated from one place to another in a shorter space of time than was required for a man to travel the distance. As to the influence exercised by the air on the infective principle of cholera, the opinion expressed was that the morbific activity of that principle was soon lost in fresh air, but that it might be preserved for an indeterminate time when the air was confined. Thus, it was the free circulation of air which rendered great deserts a most efficacious barrier against the propagation of cholera, so that there was no instance known of that disease having been imported

into either Egypt or Syria by caravans from Mecca. On the other hand, if goods of personal or domestic use, especially such as have been used by cholera patients, are taken or sent from an infected locality shut up and unexposed to free circulation of air, they might be the means of conveying the disease to any distance.

Only two of the scientific questions remained now to be settled; but these were important ones, and gave rise to long and animated discussions. They were the duration of the incubation of cholera, and the subject of disinfection. The discussion on each was very interesting, but I must of course limit myself to the conclusions arrived at. As regards the duration of incubation, the conclusions which had been adopted at Constantinople were affirmed at the present Conference by thirteen delegations, one delegation voting in the negative, and four abstaining from voting altogether. They are as follows. In almost all cases, the period of incubation, *i.e.*, the time which elapses between the moment at which the individual receives the cholera infection and the commencement of the premonitory diarrhœa, or the confirmed cholera (as the case may be), does not exceed a few days—a week at the outside. All the facts which have been brought forward to prove a longer period of incubation have related to cases which are inconclusive, either because the premonitory diarrhœa had been included in the period of incubation, or because the infection might have taken place after departure from the infected locality. Observation, moreover, shows that the duration of the choleraic diarrhœa, which has been termed premonitory (which must not be confounded with every diarrhœa met with at cholera times), does not exceed a few days; the facts which have been adduced as exceptional not being such as proved that the cases of diarrhœa so prolonged were really of a choleraic character, or were capable of transmitting cholera when the individual affected had been withdrawn from all sources of infection. The question concerning disinfection put to our Conference stood thus: "Are any means of disinfection known by which the generative or contagious principle of cholera may with certainty or with some chance of success be destroyed or deprived of its intensity?" A very short discussion rendered it obvious that it would be desirable to separate this question into two, and take separately the opinions as to the "certainty" of the operation of disinfectants, and their "probable success". This having been done, the known existence of any means of disinfection, absolutely certain in its operation,

was denied by twelve delegations and affirmed by none—seven delegations abstaining from voting; on the other hand, the existence of means of disinfection of the successful application of which there was reasonable probability was asserted by fourteen delegations and denied only by four, no delegation abstaining. Further discussion led to the following unanimous expression of opinion. "Science is not as yet acquainted with any certain and specific disinfectants; consequently the Conference recognises the great value of hygienic measures, as aëration, ablutions, cleanliness in every form, in conjunction with the employment of substances at present regarded as disinfectants.

Four sittings had been occupied with these preliminary questions, and, up to this time, as you will have seen, the Conference of Vienna was substantially of full accord with that of Constantinople. Before proceeding to the practical questions which came next for discussion, and in which the two Conferences differed, I trust I may be permitted to express my admiration of the way in which the scientific work of the Conference of Constantinople had been done. Though here and there the statements contained in its reports may be found open to correction; though now and then a difficulty is leapt over rather than solved; the fulness and carefulness of its investigations, the spirit of fairness and truth by which, as I believe, these were pervaded, and the clear and logical manner in which the conclusions are presented, deserve the warm thanks of all who have to travel over the same ground of epidemiological inquiry. The value of its suggestions of a hygienic character is admitted by all.

The Conference of Constantinople was of opinion that, as cholera is a disease of the East, and is propagated out of the East by human intercourse only, it might be practicable to keep it altogether out of Europe in future by more extended measures of quarantine by sea and land than had been heretofore attempted; and it proposed for this purpose, as the Society is aware, a scheme—admirable upon paper—which required the establishment of strict quarantine on the Red and Caspian Seas and Persian Gulf, with sanitary cordons along the Turco-Persian and Russo-Persian frontiers, and on the side of Bokhara, and which even involved complete temporary interruption, if necessary, of all communications between Egypt and the whole basin of the Mediterranean. The scheme, as a general scheme, had

never been put in execution, and was indeed in great part quite inexecutable. Its object was limited to barring the introduction of cholera from the East into Europe; it contained no provisions with reference to the extension of cholera in Europe itself.

The Conference of Vienna was not called upon specifically to review these recommendations of the Conference of Constantinople; but in the event of its agreeing (as it will have been seen it did agree) with the earlier Conference, that human intercourse was the great channel of communication of cholera, the duty next assigned to it was to consider and determine upon practical grounds, and from the present stand-point of international relations, what measures of restriction on that intercourse were practicable and were likely to be efficacious.

The questions were put before it separately as regards land quarantines or sanitary cordons, maritime quarantines, and river quarantines; the decision on the last would obviously depend on the decision with regard to the two former.

The resolution adopted by our Conference on the subject of land quarantines was as follows: That, considering that land quarantines are inexecutable and useless in face of the numerous means of communication which increase from day to day; and, considering, moreover, the grave injury which such establishments inflict on the interests of commerce, the Conference is of opinion that they should be abolished. Thirteen delegations voted in favour of this resolution, four only (those of France, Greece, Portugal, and Servia) against it; the delegations of Luxemburg and Switzerland abstained from voting; those of Egypt, Turkey, and Spain had not, at the time when this subject was before us, yet taken their places in the Conference. Up to a certain point, there was unanimity of opinion among the delegates assembled; no one contended that land quarantines could be maintained on the continent of Europe wherever a railway system existed. The controversy was as to the practicability of their maintenance so as to be effective barriers to cholera under other circumstances. A small section of the Conference held that there were circumstances under which they might be effective; and, before the above resolution was put to the vote, three amendments were successively proposed and negatived. The first of these, moved by M. Milossavlevich, the delegate for Servia, and supported by three other delegations, was to the effect that, though land quarantines were not prac-

ticable in the centre of Europe, they ought to be established wherever topographical and social conditions admitted. The second, brought forward by M. De Ring, delegate of France, and supported by four other delegations, admitted that land quarantines were, in most cases, impracticable, but proposed that any States which deemed it possible to guard efficiently their land frontiers by quarantine should be at liberty to do so. The third amendment, moved by M. Orphanides, the delegate for Greece, and supported also by four other delegations, proposed that Greece and Turkey should be excepted from the general condemnation of land quarantines, on the ground that the reasons which called for the rejection of those establishments in Europe generally did not apply to those countries. These amendments disposed of, the resolution wholly condemnatory of land quarantines was put and carried by a majority of thirteen delegations to four, as I have above stated.

The intention of this communication to the Society being narrative, it would be out of place to enter into any statement of the arguments produced to the Conference on one side or the other during this debate. But, as M. Fauvel, one of the French delegation, but who had not arrived at Vienna at the time the above discussion took place, has since taken occasion to challenge before the Académie de Médecine this decision of our Conference so far as it relates to Turkey in Asia, and to state that the land quarantine established there (in accordance with the Constantinople plan) is of incontestable efficiency, it may not be amiss if I stay for one moment to give you an account, on the unimpeachable testimony of my friend Surgeon-Major Colvill, Civil Surgeon, Baghdad, who himself a few years ago went through this quarantine, of what it really consists.

"On my way from England last autumn", says he, "to rejoin my appointment, by Russia and Persia, I had to spend ten days in quarantine on the frontier at Khanikin, and I can best give an idea of Turkish quarantine by trying to sketch what I saw. Khanikin is a town of about two thousand inhabitants. Here the river Diayalah, crossed by a stone bridge, separates Turkey from Persia, and at the Persian end a plot of ground bounded on one side by a canal was marked out as the quarantine camp; and here, for two months before my arrival, as many as five thousand travellers, Asiatic Russians, Afghans, and Persians, had at the same time been encamped. Each company, on coming in, was supposed to pitch its tents a few feet apart from the

previous arrivals, though the people mixed freely together. I arrived half an hour before midnight, and the stench from dead quadrupeds and all manner of abominations was frightful, but that was not all; for, if a person were found dead from cholera, his company had to begin the quarantine afresh, the result being that no sooner was the breath out of the dying man than his friends scraped a hole in the ground under the tent and pushed him in; while, after them, another company would arrive, pitch on the same ground, and probably do the same. In one case, the friends were in such a hurry that they left the dead man's beard protruding; the jackals came, and next morning his bare jaw-bones were sticking through the ground. No account is kept of the arrivals, and no care whatever is taken of the travellers; but that is no fault of the sanitary officers, for they have not the means. . . . . I was very much struck with the energy and perseverance of the European officer here; but what could he do, single-handed, amongst five thousand constantly coming and going men from the North, not by any means in the best of temper, and whose language he could not understand? The provisions purchased through intermediates were dear. This camp, varying from three thousand to five thousand, was simply a hot-bed of disease; while it did not prevent cholera from entering Turkey by that road, for it appeared at Kizil Robut, a village of about eight hundred inhabitants, sixteen miles on the way towards Baghdad, and for ten days five to six people died daily; and yet this camp, behind the disease, was kept up for a couple of months after that, while no quarantine was established between Kizil Robut and the interior, that is, towards Baghdad."*

The Conference had now arrived at the important subject of maritime quarantine—one on which there was found to exist, as was anticipated, wide difference of opinion, and which gave rise to long and animated discussions, prolonged altogether over nine sittings. The questions concerning it had, in view of their difficulty and importance, been referred, on the motion of Professor Hirsch, at one of the previous sittings of the Conference, to a committee for examination and report in the first instance; and it was on the report of this committee that the discussion in the Conference itself was opened. It was at this moment that the delegates from Egypt and Turkey first took their places in the assem-

* Trans. Medical and Physical Society of Bombay, N.S., No. xi, 1871.

bly, and that the French delegation, which had heretofore been represented solely by the Baron de Ring, was reinforced by the arrival of M. Fauvel and M. Proust. All States except Spain, the delegates of which did not arrive till towards the close of the Conference, were represented throughout the debates which ensued.

It would be impossible, without taking up far too much of the Society's time, to follow the course of the discussions day by day; it must suffice to give a summary of the conclusions arrived at. The resolutions which the committee in its report put before the Conference for adoption were four in number; two relating to maritime quarantine as a barrier to the progress of cholera from Asia to Europe, Europe itself being at the time free from the disease; and two relating to maritime quarantine considered as a means of preventing the communication of cholera from one European State to another. Under the former circumstances, the committee held that quarantine by sea might be useful; under the latter, that it was useless, and ought to be superseded by more practicable measures. With the view of preventing fresh incursions of cholera into Europe, the committee proposed a general approval of the quarantine which had been adopted in the Red Sea on the recommendation of the Conference of Constantinople, and the quarantines which had been recommended by the same Conference, but which had not been adopted, on the Caspian (which, however, the delegates of Russia formally announced to the present Conference the intention of their Government to establish); with the reservation that all these quarantines must be organised in a complete and satisfactory manner, in conformity with the strictest principles of hygiène. As regards European ports, when cholera had once established itself on the continent of Europe, the committee recommended the entire abolition of any measures of quarantine, and the substitution for them of a system of medical inspection differing in some of its details from, but entirely agreeing in principle with, the system in use in Great Britain, Norway, Sweden, and Denmark.

It was the latter proposals which were the great subject of contest. The chief ground taken by the committee, in recommending the abolition of quarantine in European ports, was that the unrestricted land-communication subsisting all over the Continent took from them whatever value they might otherwise be thought to have: "Contre le choléra qui a franchi les points principaux de son irruption,

les quarantaines dans les ports de l'Europe continentale sont inutiles, vu que les communications par terre leur ôtent toute valeur." The proposition thus formulated was met by an amendment moved by M. Fauvel, as follows: "Quand le choléra règne en Europe, les quarantaines maritimes perdent en général beaucoup de leur efficacité à raison des communications par terre. Cependant on ne saurait dire qu'elles soient devenues entièrement inutiles." Issue being joined on these different propositions, that of the committee was adopted by the Conference by the vote of 12 delegations against 8. The significance of the vote, however, can only be estimated by an analysis of the division. The States which constituted the majority were Great Britain, Germany, Russia, Austria, Hungary, Italy, Belgium, Holland, Denmark, Sweden, Norway, and Persia; all of them States with important maritime relations. Of the States constituting the minority, France, Turkey, Greece, Portugal, and Egypt were the only ones having any maritime relations at all; the remaining three were Luxembourg, Switzerland, and Servia. Of States of the first rank every one except France was for the sweeping away of quarantines in Europe, and France, as I shall have occasion immediately to notice, except for her Mediterranean ports, seemed scarcely prepared to defend them.

The following is briefly the proceeding that the Conference recommended should be substituted in European ports for quarantine:

Regulations for Medical Inspection.

1. Local authorities in every port for the execution of these regulations; such authorities to be kept always officially informed respecting cholera in other ports.

2. Immediate free pratique to all vessels arriving from non-infected ports, which vessels, on the oath of the captain, had not during their voyage touched at any intermediate suspected port, nor communicated with any infected ship, and on board which there had not been any case, suspected or confirmed, of cholera.

3. Medical inspection, as soon as possible after the arrival, of all ships arriving from suspected or infected ports, as well as of ships coming from non-suspected ports, but which had during the voyage touched at any suspected port, or had communication with any infected ship, or in which there had been any suspicious case of cholera sickness, or death.

4. Immediate free pratique to the ship and everything in it, whenever it appeared from this inspection that there was

no actual cholera, or suspected cholera, on board; *except* when there *had been* such cases during the voyage, under which circumstances the ship itself, the crew, and passengers, their clothes and goods, must undergo disinfection before being admitted to pratique.

5. If on the inspection there were found any case of cholera on board, or of suspected cholera, or any corpse of a person suspected to have died of cholera, (1) immediate removal of the sick (if any) to hospital, and immediate burial of corpses (if any) either in the sea with usual precautions, or inland after disinfection; (2) disinfection of the crew and passengers found to be unaffected, and of their clothing and goods; and (3) disinfection of the ship itself. To those unaffected free pratique to be accorded, with their clothes and goods, as soon as the disinfection had been accomplished.

6. Free pratique to all merchandise, except to rags and other objects of a susceptible kind, which must first undergo a radical disinfection.

The only important point in which the above regulations differ from those which have been for some time in force in the United Kingdom is in some of the provisions respecting disinfection, and especially *personal* disinfection; provisions which I think I may say it was not designed to make binding on countries which might take exception to them, and which were chiefly introduced to meet the susceptibilities of populations in countries in which quarantine had been long in use, and in which a confidence was felt by the public in the particular provisions referred to, which it would be impolitic to disregard.

In the course of the discussion which had taken place, it had become evident that from some of the States represented it was hopeless to expect, whatever the recommendation of the Conference might be, that they would be prepared to give up at once their quarantine system. On the other hand, the views held by at least some of the advocates of quarantine appeared to have undergone considerable modification tending to improvement in the practice of that system, and there was some reason to believe that the most important of the States still upholding quarantine—France—would be found ready virtually to discontinue that system in its northern ports, provided it were not called upon to do this at present in its Mediterranean ports. Under these circumstances it was determined, with a view of arriving at an understanding which might be the basis of such an international convention as the Conveners of the Conference

desired, to propose a scheme of modified quarantine which it might be at the option of the States, who should for the present decline to accept the system of medical inspection, to adopt. Accordingly, a set of quarantine regulations, abridging the period, and considerably modifying the practice, of quarantine, as heretofore carried out in Europe generally, was prepared, which might as an alternative be accepted by any State entering into the convention.

The question of river quarantine did not occupy the Conference long. It was decided unanimously (Turkey, Egypt, and Servia, abstaining from voting) that the reasons brought forward to show land quarantines to be impracticable and useless for preventing the extension of cholera were equally valid against quarantines on rivers in their course; but that to ports at the mouth of rivers, or ports which, though higher up rivers, were virtually sea-ports, as London, Antwerp, etc., the measures recommended with reference to medical inspection or maritime quarantine, as the case might be, were applicable. To certain ports of the Danube however—not sea-ports—the system of medical inspection was deemed to be applicable.

The Conference had now reached the third chief subject of its programme, viz., the consideration of the proposal for establishing at Vienna a permanent International Sanitary Commission for the more complete study of epidemic diseases and the means of preventing their spread. This subject had, like that of maritime quarantine, been referred in the first instance to a committee to consider and report on. In the original programme submitted to the Conference, the question, whether such permanent Commission might not exercise certain executive functions, had been left open; but the scheme which the committee recommended wisely discarded this idea, and was in favour of a Commission of a purely scientific character. Assuredly no proposal not so limited would have been listened to; but the members of the Conference were all of accord in desiring to see a Commission of scientific inquiry established. The practicability of doing it involved considerations which could only be determined by the several States themselves. The Conference proposed that the Commission should be a medical one, and make the etiology and prophylaxy of cholera its chief subject, with power to include within the range of its investigations other epidemics also, yellow fever and plague being especially indicated. It was proposed

that it should prepare programmes of scientific observations and researches on these subjects, to be carried on on a uniform plan by the several States. It was to keep accurate account of the progress of any of these epidemics, being kept informed by the several Governments, through their sanitary authorities, in all matters relating to the subjects under investigation. In countries in which there was no organised sanitary service, it was, with the consent of the Government concerned, to institute inquiries and investigations under its own direct superintendence. It was to publish, of course, the results of its labours. Of the various questions with regard to cholera and other epidemics as yet waiting solution which the Commission would be expected to take up, examples were presented. But other opportunities of bringing this subject before the attention of the Society will occur, should the desire of assembling the Commission ever be realised.

A fourth division of the programme had reserved to the Conference the consideration, if it saw fit, of quarantine as against other epidemics than cholera. But after thirty days of almost incessant work, the Conference was tired; its members were anxious to disperse. It spent one morning in hearing some communications—notably a most interesting one from M. Fauvel—on yellow fever, but decided to leave the consideration of the subject to the International Sanitary Commission which it desired to see convoked.

Thus ended the International Sanitary Conference of Vienna. If, as the President, the Baron von Gagern, observed in his concluding address, it did not arrive at the complete accord in practical measures which had been desired, it had at least made a first step towards it. In the Convention which will probably be signed, there is no doubt that the system of medical inspection, as replacing the antiquated system of maritime quarantine, will be accepted by all the first-class States in Europe, with the single exception of France; and it may possibly be partially accepted even by France. The four second-class maritime States at present unwilling to accept it ultimately must come in. In the meanwhile, the Conference has not already been useless in the moral support it has given to nations, like Italy and Austria, which, anxious as they are to emancipate themselves from a system which has proved not merely useless and delusive, but highly expensive and mischievous, have yet so far to take account of the susceptibilities of their populations as to render such support of value to them.

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